The Role of Out-of-School Factors on Student Performance and Educational Attainment

by

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A dissertation in partial fulfillment
of the requirements for the degree of
Doctor of Philosophy
(Public Policy and Economics)
in the University of Michigan
2017

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Dedication

To my husband Giovani and my daughter Julieta, whose unconditional love and support gave me the strength to persevere and finish.

To my parents and my sister who encouraged me at every step and showed me how love can cross borders.

Acknowledgments

I would like to thank Sue Dynarski, Brian Jacob, Jeff Smith and Hoyt Bleakley for constant support, mentorship and advice. Thanks also to Eric Chyn, Valentina Duque, Anne Fitzpatrick, Catalina Franco, Jonathan Hershaff, Joshua Hyman, Max Kapustin, Gaurav Khanna, Johannes Norling, Patricia Padilla and Ophira Vishkin, who provided countless helpful comments. I also acknowledge the valuable comments of seminar participants at the University of Michigan and the Inter-American Development Bank (IADB), and conference participants at the American Educational Research Association (AERA), the Association for Public Policy Analysis and Management (APPAM), the Association for Education Finance and Policy (AEFP) and the Population Association of America (PAA). Any errors are my own.

Thanks to my partners at the Michigan Department of Education (MDE) and Michigan's Center for Educational Performance and Information (CEPI). Chapter 2 used data structured and maintained by Michigan Consortium for Educational Research (MCER). MCER data are modified for analysis purposes using rules governed by MCER and are not identical to those data collected and maintained by MDE and CEPI. Results, information and opinions are the author's and are not endorsed by or reflect the views or positions of MDE or CEPI. The research reported in Chapter 2 was supported by the Institute for Education Sciences, U.S. Department of Education, through Grant R305E100008.

Preface

This dissertation is original, unpublished, independent work by the author, Mónica Hernández.

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Chapter I.

Fueling Violence Instead of Education? The Effect of Oil Price Shocks on Educational Attainment

Abstract

Greater local economic activity could improve the educational attainment of children by increasing the resources available to invest in education. However, higher economic activity could also result in no gains in children's education. This paper studies the role of violence in explaining this paradox, with a focus on Colombia, an oil producing country with a long-standing civil conflict. Existing evidence shows that oil price shocks fuel civil conflicts. This paper explores whether this increased violence undermines any positive effect higher oil resources might have on educational attainment. To assess how oil price shocks affect educational outcomes, this paper exploits exogenous increases in international oil prices and geographic variation in oil production. The estimates suggest that the rise in oil prices between 1998 and 2005 had a limited effect on the number of years of schooling, and whether children are behind grade for their age. Moreover, oil price shocks had small but surprising negative effects on primary school enrollment. My analysis reveals that these results might be driven by the investment of additional oil resources in social sectors other than education, and by oil price shocks fueling civil conflicts. In Colombia, instead of improving education, higher oil revenues encouraged illegal right wing paramilitary groups to steal oil resources, resulting in an 8% increase in paramilitary violence in oil municipalities.

1. Introduction

Does higher economic activity result in better educational outcomes for children? As suggested by growing evidence in developing and developed countries, the answer is “not necessarily.” In the literature, child labor is the primary explanation for this paradox: higher economic activity may increase the wages of low-skilled workers, which in turn increases the opportunity cost of studying and leads children into employment rather than education (Cascio and Narayan, 2015; Santos, 2014; Kruger, 2007; Black, McKinnish and Sanders, 2005; Duryea and Arends-Kuenning, 2003).

This paper explores another potential mechanism behind this paradox. The mechanism is violence during oil price shocks that occur in the middle of civil conflicts, which operates even in contexts where higher economic activity does not have large effects on low-skilled labor. In this paper, I show that a rise in international oil prices brought more public revenue to Colombia, an oil producing country that has suffered from a long-standing civil conflict. The rise in oil revenue did not improve educational outcomes for children, because the additional resources were not invested in education. Instead, the rise in international oil prices fueled the civil conflict, which in turn could have negatively affected education.

Oil price shocks fueled the civil conflict in Colombia because higher oil rents incentivized the violent appropriation of oil resources by non-state armed forces, as explained by Dube and Vargas (2013). In the resource curse literature, the positive effect of higher mining rents on violence has been previously found in several contexts. The resource curse broadly refers to countries rich in non-renewable natural resources that yet display poor economic performance.[1] Conflict plays a prominent role among the several proposed explanations for this paradox (World Bank, 2011). Recent studies in the resource curse literature exploit within-country variations, and have identified positive effects of the presence of natural resources on the occurrence of conflict events. (Dube

[1] Recent contributions to the resource curse literature include van der Ploeg and Poelhekke (2010), Haber and Maldonado (2011), Bruckner et al. (2012) and Wacziarg (2012), Von der Goltz and Barnwal (2014).

and Vargas, 2013; Angrist and Kugler, 2008; Maystadt et al., 2014; Buhaug and Rod, 2006 and Berman et al., 2015).

The outburst in violence brought about by oil price shocks in Colombia consisted mainly of civilian massacres, which could have affected the education of children through several pathways. First, the massacres created stress and anxiety in children, which have been shown to be detrimental to students' behavior and performance (Sharkey et al., 2012, 2014; Bowen and Bowen 1999; Delaney-Black 2002; Hurt et al. 2001). Second, the higher violence caused massive and forced population displacements, which meant loss of income and assets, gaps in school enrollment, and challenging adjustments to new settings (Uwaifo and Wharton, 2013; Stermac et al., 2013). Third, regions with high conflict levels were likely to have attracted less qualified teachers, which decreased the education quality for the children who stay in high conflict zones (Grogger, 1997; Sánchez and Rodríguez, 2010).

To assess the effect of oil price shocks on educational attainment in Colombia, I estimate a reduced-form equation of the relationship between education and oil price shocks, and complement it with an exploration of mechanisms behind the results. I use a difference-in-differences methodology, where international oil prices are interacted with the amount of oil produced in each municipality. This approach captures the exposure of each municipality to exogenous changes in international oil prices. Oil production is measured before the analysis period, so that the estimates are unaffected by any endogenous response of oil extraction to national economic conditions, which could have also affected education.[2]

To assess the effect of oil price shocks on education, this paper uses data from several Colombian sources. It employs a unique dataset that records illegal armed groups' attacks in over 950 municipalities from 1988 to 2003.[3] I use administrative data on

[2] The concern that Colombia's oil production could endogenously drive international oil prices is minor, because Colombia is a relatively small exporter of oil.

[3] The data are produced by the Center for the Study of Armed Conflict (CERAC), a private research center for conflict analysis.

school enrollment from the Ministry of Education and individual level data from the 2005 Census to measure school enrollment and educational attainment. I also use data on municipal public finances and investment by sector from the Ministry of Finance, to assess the effect of oil price shocks on municipal revenue and expenditures.

The results show that oil price shocks increased national monetary transfers to local governments in the form of oil royalties by 5.8% per year between 1998 and 2005. However, the rise in oil prices had limited effects on years of schooling and on whether a child is behind grade for his age. The higher oil prices produce small negative effects on primary school enrollment.

To explore the mechanisms behind these results, I assess the effect of oil price shocks on violence (the main channel explored in this paper) and social investment. The findings suggest that the results are explained by the investment of oil resources in social sectors other than education, together with the existence of the civil conflict which was further intensified with oil price shocks. Indeed, the evidence reveals that the rise in oil prices between 1998 and 2005 translated into an 8% annual average increase in illegal right-wing paramilitary attacks, consistent with other findings in the resource curse literature. This last result supports the main hypothesis of this paper, that violence could have an important role in the limited effect of oil price shocks on education.

I also explore the role of local labor markets in explaining the results, since increases in wages could induce children to drop out of school and enter the work force, as pointed out previously by the literature (Cascio and Narayan, 2015; Santos, 2014; Kruger, 2007; Black, McKinnish and Sanders, 2005; Duryea and Arends-Kuenning, 2003). The evidence suggests that the effects of oil price shocks on local labor markets were relatively small and imprecisely estimated. A 1% increase in oil prices is estimated to have increased wages by 0.1% and number of hours worked by 0.01%, in the average oil municipality. Both estimates are insignificant and substantively small, when compared to the effect of price shocks on commodities that, unlike oil, are labor intensive. Indeed, as Dube and Vargas (2013) show, a 1% increase in the price of coffee (a labor intensive sector), increased wages by 0.5% and number of hours worked by 0.43%, an effect that is at least five times higher than the effect of oil price shocks.

The findings of this paper are relevant to other countries that are rich in natural resources (legally or illegally extracted) and are also embedded in civil conflicts. Such is the case of Iran, Libya, Iraq or Qatar, which are big oil exporters and also have been affected by short or long term civil wars. In addition, the results are also relevant for countries that produce or transport illegal drugs and are affected by drug-cartel related violence, since a surge in illegal drug prices generates high profits that rarely benefit local residents, and instead bring high levels of violence (Angrist and Kugler, 2008). Such is the case of México, Guatemala, El Salvador and Honduras.

This paper contributes to the literature by providing an additional explanation for the null effect of natural resource shocks on education. Existing studies in civil-war free contexts have ascribed a bigger role to child labor. Kruger (2007), Black, Mckinnish and Sanders (2005) and Cascio and Narayan (2015) found that natural resource shocks increased the wages of low-skilled workers. The higher wages increased the opportunity cost of studying, inducing children into employment and out of school. Yet, when the price shocks happen in the context of a civil conflict, this paper provides evidence of violence as an alternative mechanism.

In exploring a different pathway, this paper also contributes to the literature by bringing together insights from two different research fields. The first field is the resource curse literature previously mentioned, which showed that natural resources increase conflicts. The second field is the literature on the negative effects of civil conflicts on human capital accumulation. Studies in Perú (León, 2012), Rwanda (Akresh and de Walque, 2010), Germany (Akbulut-Yuksel, 2008), Guatemala (Chamarbagwala, and Morán, 2011) and Colombia (Duque, 2014; Sánchez and Dominguez, 2012) find that adults who were exposed to civil conflicts during childhood achieve between 0.3 and 1.1 less years of education than non-exposed ones. The findings in these two strands of the literature are consistent with the hypothesis of this paper, that any positive effect of natural resource shocks on education, can be nullified by the negative effects of the civil conflict.

Finally, this paper provides an additional explanation for the limited effect of additional local government revenue on social welfare in low-income countries. The existing literature has attributed the lack of an effect to low institutional quality[4] (Robinson et al., 2006; Mehlum et al. 2006) or to local governments obtaining most of their resources from external sources, such as natural resources rents[5] (Gadenne and Singhal, 2014; Martinez, 2015; Paler, 2013; Martin, 2014). This paper suggests yet another mechanism, that can even amplify the phenomena pointed to by the existing literature. If natural resource shocks fuel civil conflicts in the context of weak institutions, then local governments would be even less able to raise social welfare in the presence of these shocks.

The remainder of this paper is organized as follows. Section 2 gives background information on oil resources and the civil conflict in Colombia. Section 3 describes the different sources of data used. Section 4 presents the estimates of the effect of oil price shocks on school enrollment and public finances. Section 5 follows by presenting evidence of violence as a mechanism, as well as other possible mechanisms. Section 6 concludes.

2. Background: The Context of Oil Price Shocks and Education

This section explains the lack of an effect of oil price shocks on education, in the context of the civil conflict in Colombia. The section begins by explaining how oil resources are transferred to oil municipalities and why these resources should be expected to improve education. The discussion then turns to a brief description of the civil conflict in Colombia, and how oil price shocks have fueled it. Finally, the relationship between the

[4] Examples are weak accountability systems, low state competence, dysfunctional democracies, weak rule of law and unsatisfactory protection of property and civil rights.

[5] In particular, this strand of the literature hypothesizes that tax-payers hold local governments accountable only for how they spend tax resources, leading local politicians to often spend external resources inefficiently or for their own benefit.

civil conflict and educational attainment in Colombia is examined, in the context of the literature of the effect of violence on education. The conclusion of this section is that illegal paramilitary groups have used the oil money transferred to municipalities to fund massacres of civilians, that spread fear, led to forced displacement and created gaps in school enrollment.

2.1. The sources and uses of oil royalties in Colombia

Oil royalties represent a share of oil taxes that the national government transfers to oil municipalities, in exchange for the exploration of oil in their territory. The central government taxes 8-25% of oil profits, depending on the level of production. Eighty percent of the oil tax revenue is preserved in a stabilization fund. Of the 20% remaining, the central government keeps between 8-32% and transfers 47-52% to departments (states) and 12-32% to municipalities, depending on the level of production. As a result of the transfer, municipalities receive between 2-6% of the oil tax revenue collected, depending on the level of oil production.

The transfer of oil resources to oil municipalities is the result of a major decentralization process that happened in the early 90s, which delegated to local governments the responsibility of implementing local social programs. To perform the new duties, the decentralization provided municipalities with additional financial resources, among which are oil royalties. The resources are transferred to the municipalities under the condition that the money would be invested in specific social sectors. The majority of oil royalties (75%), in particular, should be spent on health, education, and water sanitation, until some minimum standards are achieved.[6] Although oil royalties are earmarked, municipalities have discretion on how to spend the resources within those sectors (e.g. whether to use the resources to build new schools or to pay teachers' salaries). Given that education is one of the sectors mandated to receive oil royalties, it

[6] Once those benchmarks have been satisfied, the local government can spend up to 90% of the transfers on projects prioritized in their development plan.

would be expected that oil price shocks bring gains to education outcomes in oil municipalities.

2.2. The Colombian civil conflict and how it was fueled by oil price shocks

The Colombian civil war has its roots in *La Violencia*, a violent confrontation between the two traditional political parties, the Liberals and the Conservatives, which took place from 1948-1958. The power sharing agreement that ended this confrontation was perceived to exclude representation of the rural poor and other political forces. This perception of exclusion contributed to the formation of revolutionary leftist guerrilla groups, which launched a communist insurgency during the 1960s. In response to guerrilla extortion and violence, the 1980s saw the rise of a first generation of right-wing paramilitary groups, formed by drug lords and the rural elite.

The conflict remained low intensity during the 1980s, but escalated sharply during the late 1990s for a number of different reasons, including the coalition of paramilitary groups as a unified group. Before 1997, the paramilitary operated as isolated cells with low intensity violence, but they combined into a unified group in 1997 (AUC[7] is its acronym in Spanish) in response to an acceleration of left-wing guerrilla violence. This formation of the coalition coincided with a sharp increase in paramilitary violence (Figure 2) and allowed the unified group to emerge as an important armed group, which was estimated to have over 15,000 fighters at its peak in 2001-2003. Paramilitary members attained a vast power that embedded them in the government military (Dube and Vargas, 2013), local politics (López, 2010; Acemoglu et al., 2013) and the regional economy (Indepaz, 2008). This wide sphere of influence enabled the subjugation of entire populations (Restrepo and Spagat, 2004).

During the sample period studied in this paper, the conflict can be characterized as three-sided, with the government military, the guerrillas and the paramilitary groups

[7] *Autodefensas Unidas de Colombia* or the United Self-Defense Groups of Colombia

fighting one another[8]. However, for the purposes of this paper, I focus only on the paramilitary, which is the illegal armed group that has syphoned oil royalties from local governments under the threat of force. The greater amounts of money transferred to municipalities that produced oil increased the paramilitary's incentives to exert control. These incentives were exacerbated by the fact that local governments had low capacity to fight theft and extortion, leaving local leaders vulnerable.

The media has documented how the paramilitary threatened government officials to grant public contracts to particular firms and then extorted executives in those companies to extract the oil rents (Semana, 2007). Another form of extortion was to kidnap and murder mayors, to directly extract oil and gas royalties from municipality coffers (El Tiempo, 2007). Oil price shocks fueled the civil conflict, because paramilitary groups used violence to extort the oil money and funded their war activities with this money. Indeed, as Figure 1 shows, paramilitary violence in municipalities that produced oil increased in years of high oil prices, compared to municipalities with no oil production[9].

2.3. How does a more acute civil conflict limit educational attainment?

The previous subsections explained how oil price shocks ended up fueling the civil conflict in Colombia by financing the war activities of the paramilitary groups. Here I address how this increase in violence affected children's educational outcomes and how violence in the context of the civil conflict compares to other forms of crime.

[8] Typically, however, the state and the paramilitary groups have been unofficially allied in fighting the guerrillas.

[9] A first glance to Figure 1 could wrongly lead to conclude the opposite (that increases in paramilitary violence in oil municipalities led to increases in oil prices, and not vice versa). However, this scenario is very unlikely. Oil prices started to increase for exogenous reasons on 1998 and this happened, by coincidence, one year after the collusion of the paramilitary groups. The 1997 collusion was the response of the paramilitary to the rising power of the left-wing guerrillas throughout the Colombian territory. After this collusion, paramilitary violence significantly increased, even more so in oil municipalities, following the exogenous increases in oil prices.

Paramilitary groups used as a war strategy civilian massacres, defined as killings of four or more defenseless people with some selectiveness regarding the people killed or the place where they were killed. These massacres were central for the paramilitaries to control local populations. The violent massacres deterred civilians from supporting the left-wing guerrillas, therefore cutting the provision of food, medicine and messages to paramilitaries (Kirk, 2003).

Children living in the municipalities where massacres happened were exposed to violence because, although small[10], massacres tended to occur in rural low density population areas (Restrepo and Spagatt, 2004)[11]. As a consequence, children were likely to witness or hear about massacres, combats, shootings, bombs or kidnappings, and may even have had family members who died or were injured.

Paramilitary attacks were high in frequency and were spread throughout the country. In fact, from 1988 to 2005, paramilitary groups committed 1,137 attacks, 648 of which were massacres. One third of the 1,100 municipalities suffered at least one attack, specifically, 339 municipalities. The attacks perpetrated by the paramilitary increased dramatically after the formation of AUC in 1997, and was manifested both in more massacres as well as other types of attacks (Figure 2).

The frequent and intense paramilitary attacks spread fear and caused massive forced displacements. Many families fled to small, medium and large urban areas in search of security, making Colombia the country with the second largest population of internally displaced persons in the world, after Sudan (UN Security Council, 2009). These factors profoundly affected children's education for several reasons.

First, the higher violence spread fear amongst the population, producing stress and anxiety in children, which has been shown to negatively affect student's socio-emotional,

[10] There was an average of six civilians killed in each paramilitary massacre (Restrepo and Spagatt, 2004)

[11] Some of the massacres involved stealing property and burning houses and businesses, and were complemented with public road blockages, to prevent the provision of food to the populations (Grupo de Memoria Histórica, 2013).

behavioral and cognitive domains (Sharkey et al. 2012; Osofsky 1999; Shahinfar, Kupersmidt, and Matza 2001; Margolin and Gordis 2004; Bingenheimer et al. 2005). Similar to other traumatic experiences (such as maltreatment), exposure to local violence and danger are also associated with lower performance, cognitive skills, grade point average and school attendance (Sharkey et al. 2014; Bowen and Bowen 1999; Delaney-Black 2002; Hurt et al. 2001).

Second, the paramilitary attacks created sudden and large flows of families migrating out of violent areas. With the migration, families lost their income and assets, which are factors that have been shown to affect negatively children's schooling (Thomas et al., 2004; Duryea, Lam and Levinson, 2007; Jacoby and Skoufias, 1996). Moreover, children transited to new and unfamiliar school systems. This transition can be challenging and may create gaps in school enrollment, particularly if the pre-migration academic experiences have been unstable and interrupted (Stermac et al. 2013; Uwaifo and Wharton, 2013). Formal education for some children may be curtailed due to lack of provision of education in the new location (Hart, 2013) or might be delayed because the placement process requires sensitive and appropriate determination of the proper grade level, as well as an assessment of any learning disabilities or psychological difficulties (Kapreilian-Churchill, 1996; Stermac et al. 2013). Conversely, it might happen that forcible displacement could open up the possibility of higher quality schooling that had been unavailable previously (Hart, 2014). The quality of education in the areas where families relocate could be higher, because displaced families tend to flee to urban areas, where the provision of public services tends to be higher and better.

Third, violence may limit the local supply of education. Acute episodes of violence can cause destruction of physical infrastructure (e.g. school, buses, libraries) that are crucial inputs for children's education (Barrera and Ibañez, 2004). In addition, teachers may, on the one hand, avoid taking or leave positions in cities or towns with high conflict levels (Sánchez and Rodríguez, 2010) or, on the other hand, demand higher salaries to teach in municipalities highly affected by the war (Grogger, 1997).

Violence in the context of an armed conflict (such as paramilitary attacks) needs to be studied as distinct from other forms of violence (such as urban crime). Indeed, violence due to armed conflict differs from other forms of violence in the level of cruelty and scope of the methods used. During civil conflict, combatants use pillage, predation, extortion and deliberate violence against civilians to acquire control of territories, lucrative assets, trade networks and labor (Villar-Márquez and Harper, 2010). The latter is particularly salient in Colombia, since minors have been constantly recruited by illegal armed forces. These differences mean that the education effects of civil conflict can be more acute than the educational effects of urban crime.

Although it may be more severe, civil conflict does share some features of urban crime, since criminal groups use murder to gain control in both contexts. The Colombian civil conflict, in particular, resulted in higher homicide rates during its most acute period, as the illegal armed groups (paramilitary in particular) used murder to control the population. Indeed, as Figure 3 shows, after the creation of the AUC in 1997, the rise in homicide rates in the late 90s coincided with a sharp rise in paramilitary homicides.

3. Empirical Strategy: Difference-in-Differences Methodology

This section explains the empirical strategy used to assess the effect of oil price shocks on education outcomes. The method consists on interacting the exogenous international oil price with the amount of oil produced in each municipality in the baseline year (1988). This section explains why this method alleviates possible endogenous correlations between oil production and education, and rules out that international oil prices could be endogenously driven by Colombia's oil production.

The empirical strategy follows a difference-in-differences estimator, by assessing whether changes in oil prices affect the education outcome disproportionately in municipalities that produce more oil. This strategy has been used in recent studies that investigate the effect of commodity price shocks on diverse outcomes in Colombia (Dube and Vargas

(2013), Carreri and Dube (2014), Santos (2014), Idrobo, Mejia and Tribin (2014), Martinez (2016)).

In this approach, time variation stems from movements in international oil prices, which is exogenous to Colombia's production as the country is a relatively small exporter of oil. According to the US Energy Information Administration, Colombia is the 18th largest exporter of oil with less than 1% of the world exports. Figure 1 shows there was significant variation in oil prices during the sample period (1996-2005): after following a decreasing trend until 1998, oil prices increased steadily until the end of the analysis period.

Cross-sectional variation comes from the distribution of oil production across different municipalities in the baseline year (1988), which alleviates the potential endogeneity concerns for two reasons. First, because oil production is determined by the (exogenous) spatial distribution of oil reserves. Second, because production is measured in the baseline year, which means it does not reflect potential endogenous oil discovery efforts or extraction rates correlated with education over the period of analysis.

3.1. Specification at the individual level

The specification to estimate the effect of oil price shocks on individual education outcomes can be represented by the following equation

$$y_{imrt} = \beta_0 + (Oil_m \times OP_i)\gamma + X_i^{'}\beta_1 + Z_{mt}\beta_2 + \alpha_m + g_t + \mu_r(t) + \varepsilon_{imrt} \quad (1)$$

where y_{imrt} is the education outcome for child i who was born in municipality m, region r and year t; α_m are municipality of birth fixed effects; g_t are year of birth fixed effects. X_i are child level characteristics, that include indicators for child's gender and race, head of the household gender and level of education, household size and whether the household lives in urban or rural area. Z_{mt} are time-varying municipality controls, that include log population in municipality of birth m in year of birth t and $\mu_r(t)$ are region of birth linear trends for Colombia's four major regions[12] and also for coca producing municipalities.

The interaction term $Oil_m \times OP_i$ captures the exposure of child i to oil price shocks during different stages of his life, and its corresponding coefficient γ is the main parameter of interest. In a first specification, I estimate the effect of the lifetime exposure to oil price shocks on educational outcomes, where the main term of interest takes the form

$$(Oil_m \times OP_i)\gamma = \gamma \sum_k (Oil_m \times OP_{t+k}) \quad (1.1)$$

where Oil_m is the oil production level in the municipality of birth m during 1988 and OP_{t+k} is the log oil price in year $t+k$. The subscript k denotes age, so the interaction term $Oil_m \times OP_{t+k}$ captures the exposure of child i, who was born in year t, to oil price shocks when he was age k[13] and the sum $\sum_k Oil_m \times OP_{t+k}$ is the accumulated exposure of child i to oil price shocks over his lifetime.[14] In this specification γ measures the effect of a lifetime exposure to oil price shocks on educational outcomes.

A natural question that arises is whether the effect of being exposed to oil price shocks differs depending on the age of exposure. To answer this question, I estimate a second specification, where the main term of interest takes the form

$$(Oil_m \times OP_i)\gamma = \gamma_1 \sum_{k=0}^{6} Oil_m \times OP_{t+k} + \gamma_2 \sum_{k=7}^{11} Oil_m \times OP_{t+k} + \gamma_3 \sum_{k=12}^{17} Oil_m \times OP_{t+k} \quad (1.2)$$

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[12] The regions are Andean, Caribbean, Southeastern and Pacific.

[13] This term takes zero if the child had not yet achieved age k when he was observed in 2005.

[14] A natural concern when several log terms are added like in this expression, is that the different terms would cancel with each other. This is because l*og(x)* can be both positive (for large values of x) or negative (when x is very small). However, this is not a major concern because oil prices in the period of analysis were high enough, such as log oil price was always positive and large.

where the sum $\sum_{k=a}^{b} Oil_m \times OP_{t+k}$ is the accumulated exposure of child *i* to oil price shocks between ages *a* and *b*. In equation (1.2), γ_1 captures the effect of exposure to oil price shocks from ages 0 to 6, γ_2 from ages 7 to 11 and γ_3 from ages 12 to 17.[15]

In equation (1), the inclusion of municipality of birth fixed effects α_m takes care of possible sources of bias coming from time-invariant differences between oil and non-oil municipalities. For example, as shown in Table 2, oil municipalities tend to be more urban, and places with high urbanicity levels tend to have higher quality institutions that supply better public goods (such as education). The municipality fixed effects control for observed and unobserved differences between the oil and non-oil regions that are constant over time.

In addition, the year of birth fixed effects g_t control for time trends, absorbing aggregate shocks that could bias the results. For instance, the change in the trend of oil prices (which decreased until 1998, and increased steadily afterwards) coincided with a historical economic recession in 1999, that decreased school enrollment rates. By including year fixed effects, I use local rather aggregate variation to identify the effect of oil price shocks on the education outcome.

The inclusion of region linear time trends $\mu_r(t)$ accounts for potential omitted variables since oil may be concentrated in particular regions, and education may be trending upward in these locations based on other factors such as varying economic growth rates. The trends in the coca and non-coca municipalities also mitigate potential omitted variable bias since coca presence may be correlated with oil presence, and both coca planting and government eradication efforts increased dramatically during the 1990s, either of which may have caused violence to trend upwards in the coca area.

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[15] The most flexible specification consists of estimating the effect of being exposed to oil price shocks each year of life, which means estimating the coefficient for each term Oil_m x OP_{t+k}. However, these terms are highly collinear with each other making the estimation unfeasible.

Oil prices are measured in logs, so the coefficients in the interaction terms can be interpreted as semi-elasticities, weighted by oil production in the municipality. Standard errors are clustered at the municipality of birth level, to account for possible correlation of errors within municipalities across time.

3.2. Specification at the municipality-year level

The specification at the municipality-year level is used to explore the mechanisms behind the lack of an effect of oil price shocks on education, by estimating how oil price shocks affect public finances and paramilitary violence. This specification is also utilized to confirm that the effect of larger oil resources on education is limited, using school enrollment data aggregated at the municipality level.

The specification can be represented by the following equation

$$y_{mrt} = \beta_0 + \lambda(Oil_m \times OP_t) + Z_{mt}\beta_1 + \alpha_m + g_t + \mu_r(t) + u_{mrt} \qquad (2)$$

where y_{mt} is the outcome in municipality m in year t; Oil_m is the oil production level in municipality m during 1988; OP_t is the log of international price of oil in year t; α_m are municipality fixed effects; g_t are year fixed effects; and Z_{mt} are time-varying municipality controls, that include log population. $\mu_r(t)$ are region linear trends for Colombia's four major regions and also for coca producing municipalities. In equation (2), λ captures the differential effect of the oil price on public finances or school enrollment in municipalities producing more oil.

4. Data

This section describes the several sources of data used to study the effect of oil price shocks on education. It explains how the different databases are merged together to create the sample of study. It shows that the sample of study is representative of rural areas, where the Colombian civil conflict is more acute.

4.1. Armed conflict data at the municipality level

The armed conflict data comes from a detailed event-based database for the period 1988-2005, with broad coverage in rural areas where the conflict is more acute. It was constructed by the Conflict Analysis Resource Center (CERAC is its acronym in Spanish) and provides the main independent variable of interest in this study, the count of paramilitary attacks per municipality and year.

The primary sources of this database are a network of priests from the Catholic Church with representation in almost every municipality in Colombia, and over 25 newspapers with national and local coverage. The dataset, which is described thoroughly by Restrepo, Spagat, and Vargas (2004) and Dube and Vargas (2013), is constructed on the basis of the events listed in the annexes of periodicals published by two Colombian human rights NGOs (CINEP and Justicia y Paz). Since the magazines include reports from Catholic priests, who are often located in rural areas that are unlikely to receive press coverage, the database has broad representation in rural areas. For every event the conflict dataset records its type, the date, location, perpetrator and victims involved in the incident.

This paper uses the total count of paramilitary attacks per municipality and year registered in this database, to construct the main independent variable of the analysis. Paramilitary attacks in the CERAC database are defined as violent events implemented solely by paramilitaries without effective resistance[16] and include fire upon other armed groups and attacks against the civil population. The attacks against the civilians are overwhelmingly massacres[17], and to a lesser extent are incursions (attacks on villages), check points (search of vehicles and questioning occupants) and public road blockages (Restrepo and Spagatt, 2004).

The count of paramilitary attacks is available for 998 municipalities, of the (approx.) 1,100 municipalities in Colombia. The municipalities not included in the sample

[16] CERAC has an opposite category for attacks: clashes, which are fights between two armed groups. Attacks that lead to clashes are classified as simple clashes.

[17] Between 1988 and 2003, 70% of the attacks against the civil population implemented by paramilitaries were massacres (Restrepo and Spagatt, 2004).

correspond mostly to the largest metropolitan areas in Colombia. These areas are omitted from the CERAC sample because the insurgency is concentrated in rural areas, making the dynamics of the conflict very different between rural and urban areas[18].

4.2. Education outcomes at the individual level coming from the 2005 Census

The main dependent variables of the analysis, individual education outcomes, come from the 2005 Census collected by the Colombian Bureau of Statistics (DANE is the acronym in Spanish). The education outcomes studied are school enrollment, whether the child is behind grade for his age[19] and the number of years of schooling.

The 2005 Census data was obtained from IPUMS-International (Minnesota Population Center 2011). It includes a representative sample of four million individuals, accounting for 10% of the total population, and provides rich demographic and socio-economic information on each member of the household. One virtue of using the Census is that it reports the municipality and year of birth for each individual. This allows me to approximate each child's exposure to oil price shocks, by measuring the exposure of his birth municipality to oil price shocks. Using the municipality of birth, as opposed to the municipality of residence, also allows me to assess possible endogenous household migration in response to oil price shocks.

Although Colombia has approximately 1,100 municipalities, they are grouped into 532 census-units, which I also call grouped municipalities. The census-units are created by DANE, such that small municipalities (with less than 20,000 inhabitants) are grouped together with their neighboring municipalities, to form larger entities[20]. This paper includes children who were born in 496 of the 532 grouped municipalities, given the

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[18] The CERAC data was obtained from the Dube and Vargas (2013) replication package. Therefore, the paramilitary violence information is available for the 988 municipalities included in their estimation sample. Besides excluding large urban areas, the authors also exclude areas where violence information was missing some years, to yield a balanced panel of municipalities.

[19] A child is behind grade for his age if his age minus his years of schooling is 8 or higher.

[20] Combining municipalities in this manner does not represent a problem for data analysis since each municipality in the grouped areas is clearly identified.

availability of data on the armed conflict. The children excluded from the analysis are those who were born in a group of municipalities with incomplete armed conflict information[21], meaning large cities and their metropolitan areas, as described in section 4.1.

4.3. Education outcomes at the municipality level coming from administrative data

The secondary dependent variable of the analysis, school enrollment at the municipality level, is measured using administrative data coming from the Ministry of Education. The data records enrollment in primary and secondary schools by municipality and year, for the period 1996 to 2005. This information was obtained from the repository of municipality level data compiled by the Center of Economic Studies (CEDE for its acronym in Spanish) at Universidad de los Andes.

Enrollment rates in primary and secondary school are obtained by dividing the number of students in primary (secondary) school, by the population in age of attending primary (secondary), e.g. children 7-11 (12-17) years old. The population by age range was also obtained from the CEDE database, and was constructed based on the Census 2005 information and projections of municipality population by year.

4.4. Public finances data and other municipality information

The public finances data is available for the 1996-2005 period, and comes from the municipality accounting sheets provided by the National Planning Department (DNP for its acronym in Spanish). It records the transfers from the national government to the municipality, which include oil royalties, as well as municipality social expenditures. This information allows estimating the effect of oil price shocks on local government revenue and expenditures.

The public revenue data records most oil transfers under a specific category that registers national transfers with a mandatory destination. Oil royalties are classified in

[21] A group of municipalities has incomplete armed conflict information if it has at least one municipality with no armed conflict data.

this category because they are transferred to municipalities under the condition of spending them on the health, education and water sanitation sectors[22]. The data also includes other sources of revenue, such as municipality tax and non-tax revenue, transfers from the departments (states) and discretionary transfers from the national government.

Local social expenditures, on the other hand, are recorded under a social investment category, that includes teachers' wages, school materials, and hospital expenditures, among other things[23]. This social investment category belongs to a broader investment category in the public finances sheets that also records investment in physical assets, labeled as fixed capital accumulation. The public expenditures data also includes other types of expenditures, such as local government operational costs and debt interest payment.

The public finances data is complemented with a separate source of information that records municipality investment by sector, used by the DNP to feed the accounting sheets. This data includes municipality investment in education, health, water sanitation and transportation, among other sectors. The data does not discriminate between investment in physical assets (fixed capital accumulation) and social investment, so it is comparable to the broad investment category in the public finances sheets[24].

Figures 1 and 2 show that oil and non-oil municipalities differenced from each other in terms of public revenue. In years with high oil prices, oil municipalities displayed higher transfers from the national government with a mandatory destination (which include oil royalties). However, they did not display higher social expenditures, compared to non-oil municipalities.

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[22] Seventy five percent of the transfers should be spent on health, education, water and sewage, until some minimum standards are achieved. Once those benchmarks have been satisfied, the local government can spend up to 90% of the transfers on projects prioritized in their development plan.

[23] These social operational costs are classified as "investment" following the recommendations of the IMF for local public accounting (DNP, 2014; IMF, 2001).

[24] Total investment reported by the two sources of data has similar trends and magnitudes. There are some small differences, probably due to the process followed to clean the sector investment database.

The rest of the data at the municipality level is combined from several sources. Oil price data comes from IFS (International Financial Statistics, IMF) and oil production in 1988 comes from the Ministry of Mines and Energy. Municipality population by year is calculated based on information coming from the 2005 Census and the Colombian Bureau of Statistics (DANE for its acronym in Spanish).[25] Finally, coca cultivation in 1994 by municipality comes from the National Department of Narcotics (DNE for its acronym in Spanish); this data is used to control for linear trends in coca municipalities.

4.5. Sample and Summary Statistics

This paper uses two estimation samples to assess the effect of oil price shocks on education and its underlying mechanisms: the first sample is at the individual level and the second one is at the municipality-year level. Both samples are representative of rural and small urban areas, where the conflict is more acute.

4.5.1. Individual Level Sample

The individual level sample is based on the 2005 Census and is used to assess the effect of oil price shocks on individual education outcomes. The sample consists of 631,285 children who were 7-17 years old in 2005, who belong to the 1988-1998 birth cohorts. These cohorts were chosen such that the children would be old enough to attend school, but also young enough so they would have been born on or after 1988, which is the year when oil production is measured.[26] Using these cohorts also allows me to measure exposure to violence since birth, because the armed conflict starts in 1988.

The sample excludes children who i) were born in a census-unit (or group of municipalities) for which information on the armed conflict is incomplete; or ii) have no information on some of the control variables included in the regressions. Table A1 in the

[25] Municipality population is estimated using the share of the municipality in the department population in the 2005 Census (which is the last Census available) and the projections of department population by year, published by DANE.

[26] It is important for children in the sample not to be born before 1988, because otherwise oil production could have responded to economic conditions in the first years of their lives, making it an endogenous production measure.

Appendix compares children in sample with those out of sample, dividing the latter according to the reason for the exclusion (i or ii). The Table reveals that most of the children not in the sample are excluded on the basis of (i). The excluded children tend to mainly live in urban areas, consistent with the fact that large cities and their metropolitan areas are excluded from the armed conflict data (as explained in Sections 4.1 and 4.2). This difference between children in and out of sample implies that the results of this study are representative of rural and small urban areas, where the conflict is more acute.

To measure the exposure of children to oil price shocks, I use the oil production level in their municipality of birth as of 1988. The census does not provide exact information on the municipality of birth, but instead on the group of municipalities (census-unit) where the child was born. Therefore, I create an aggregated measure of oil production in the census-unit of birth, by calculating the average oil production in 1988 in the group of municipalities where the child was born, weighted by the municipality population in 1988.[27]

The exposure of children to paramilitary attacks each year of their lives is calculated in a similar way to exposure to oil price shocks. Specifically, paramilitary attacks at age a are calculated as the average paramilitary attacks that happened when the child was age a in the group of municipalities where the child was born, weighted by the population in each of these municipalities. It is possible to measure the exposure to violence since age zero because the children in the sample were born on or after 1988.

Table 1 presents statistics of children according to whether they were exposed to oil price shocks or not (e.g. whether they were born in a municipality that produced oil in 1988). It reveals that the educational outcomes of both type of children are remarkably similar and even tend to be slightly worse in oil municipalities, since children in oil

[27] It is necessary to weight the average by the population in the municipality, because the probability that the child was born in a particular municipality with a specific oil production is proportional to the population in that municipality.

municipalities attend school at rates one percentage point lower, and one percentage point more of them are behind grade for their age (Column 3). The table also shows that children living in oil municipalities are exposed to paramilitary violence at much higher rates. The proportion of children ever exposed to a paramilitary attack is 16 percentage points higher in oil municipalities. These children have also been exposed to 6 more paramilitary attacks over their lifetime.

4.5.2. Municipality-year level sample

The municipality-year level sample consists of a balanced panel of 936 municipalities with information on paramilitary violence, school enrollment and public finances for the period 1996-2005. Although this sample is mainly employed to assess the effect of oil price shocks on public finances and paramilitary violence, it is also utilized to provide further evidence of the lack of an effect of oil price shocks on school enrollment.

The municipality-year level sample differs from the individual level sample, in that the municipalities are not aggregated in census-units (or groups of municipalities). Instead, the original municipality boundaries are preserved to take the maximum advantage of the underlying geographic variation in the data. Also, while the individual level sample utilizes violence information since 1988, the municipality-year level sample includes information since 1996, when the school enrollment information at the municipality level starts to be available.[28]

Similar to the individual level sample, the municipality-year level sample excludes large urban areas, because there is no paramilitary violence information for them[29]. In addition, it excludes very small rural areas with school enrollment data missing some years. Therefore, this sample is approximately representative of medium rural and small

[28] Another reason for this choice is that the public finances data did not include oil royalties until 1996, when the fiscal decentralization process started operating (Section 2.1. describes this process in more detail).

[29] Our source of violence data (CERAC) excludes large metropolitan areas because the insurgency is concentrated in rural areas, making the dynamics of the conflict very different from the cities.

urban areas[30]. The final estimation sample contains 9,360 municipality-year observations over the 10 year period.

Table 2, which presents summary statistics of the municipalities in the sample by oil production, reveals that oil municipalities received higher transfers from the national government (in the form of royalties for the production of oil) during the analysis period (1996-2005). These transfers (labeled public transfers with mandatory destination), were on average 3 times higher in oil municipalities than in non-oil ones. Figure 1, which presents the trends in government transfers by oil production, also shows that the increases in government transfers to oil municipalities were higher in years when oil prices increased.

Table 2 suggests that public social expenditures and primary school enrollment rates were higher in oil municipalities, which could be equivocally interpreted as the effect of higher oil resources on social welfare. However, this relationship could instead reflect the existence of other confounding factors that are correlated with education. As Table 2 shows, oil municipalities tend to be larger and more urban, which are factors that are related with higher education levels. The existence of these confounding factors highlights the importance of including municipality fixed effects in the regressions and controlling for time-variant municipality characteristics[31].

Table 2 also reveals that paramilitary violence was significantly higher in the oil region, both in terms of the incidence and the intensity of the exposure to violence. In terms of

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[30] This sample is arguably useful to study the relationship between violence, oil production and education. The exclusion of large urban areas is not a concern because the dynamics of the conflict are very different between rural and urban areas. The exclusion of small rural areas is not concerning either, because these municipalities do not produce oil, nor do they have significant levels of paramilitary violence. Table 14. in the Appendix, which compares the municipalities in and out of the sample, presents evidence of these facts.

[31] According to Table 1, the individual level data reveals that the overall school enrollment rate is remarkably similar between oil and non-oil municipalities. This would seem contradictory with the results in Table 2, which reveals that oil municipalities have higher primary enrollment rates. However, there is no such contradiction, because oil municipalities also have lower secondary enrollment rates (Table 2). When these two contrary forces combine (higher primary rates, together with lower secondary rates), the result in similar overall enrollment rates in oil and non-oil municipalities.

incidence, the percent of municipalities that experienced at least one paramilitary attack was almost two times higher in the oil region (66% vs. 35%) during the analysis period. In terms of the intensity, oil municipalities suffered 4 times more paramilitary attacks than non-oil municipalities (4.2 vs. 1 attack). These stark differences seem to be driven by increases in oil prices, since years with high oil prices were followed by higher paramilitary attacks in the oil region, as revealed by Figure 1[32].

5. Results: The Lack of an Effect of Oil Price Shocks on Education Outcomes

This section describes the results of estimating the effect of oil price shocks on education. It shows that the increase in oil prices between 1998 and 2005 had limited effects on whether a child is behind grade for his age and on years of schooling. More surprising, increases in oil prices had small but negative effects on primary school enrollment in oil municipalities.

Table 3 presents the results of estimating Equation (1) on children's school enrollment. Columns 1 and 2 show the results for the entire sample (children 7-17 years old in 2005), while columns 3 to 6 divide the sample according to whether the child is in primary school age (7-11) or in secondary school age (12-17). Column 1, which presents the results for lifetime exposure to oil price shocks (as specified by equation 1.1), reveals that increases in oil prices have no effects on school enrollment since the coefficient in Column 1 is small and statistically insignificant. Furthermore, dividing the sample by age reveals that oil price shocks had small but surprisingly negative effects on enrollment of children of primary school age (Column 3).

When the lifetime results are disaggregated by age of exposure in Columns 2, 4 and 6, it is not possible to identify a clear pattern. The sign of the coefficients is negative in some instances and positive in some others, depending on the sample used. Therefore, the

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[32] See footnote 9 for relevant information.

evidence is not conclusive with regards to the effect of exposure to oil price shocks at different ages.[33]

Tables 4 and 5 display the results when the outcome considered is years of schooling and whether the child is behind grade for his age. The results for the latter are similar to those found for school enrollment, in that oil price shocks have limited effects on years of schooling and whether the child is on track in school (Column 1, in Table 4 and 5). When the sample is split according to age, however, it is found that oil price shocks seemed to help children of secondary school age to be on track in school and to increase their years of schooling (Column 5 in Tables 4 and 5). Nonetheless, these are small effects as explained in the next couple of paragraphs.

To understand the magnitude of the coefficients in Tables 3 to 5, I calculate the annual change in education outcomes caused by a lifetime of exposure to oil price shocks. To calculate this annual change, consider the lifetime coefficient in Column 1 of Table 3. This coefficient is multiplied by the average oil production in oil municipalities (0.072 hundred thousand barrels/day), which yields the semi-elasticity of school enrollment to oil prices in the average oil municipality. This semi-elasticity is then multiplied by the change in log oil prices between 1998 and 2005 (1.37), when oil prices tripled. The calculation reveals that school enrollment decreased 0.006481 percentage points in an 8-year period, translating into an average change of -0.000814 in school enrollment per year, in the average oil municipality. In practical terms, this represents a zero effect.

For illustration purposes, the interpreted coefficients are plotted in Figures 6 to 8, together with their 95% confidence level intervals. The Figures reveal that oil price shocks had limited effects on all three educational outcomes, since the confidence intervals include zero. There are two exceptions to this pattern, as described before. One is school enrollment of children of primary school age, which decreases when oil prices

[33] The shift in the sign of the coefficients might be related with the fact that the variables for exposure at a particular age are collinear. This problem was pointed out previously when describing the empirical strategy.

increase. These negative effects are very small (0.0052 percentage points) but are statistically significant. The second exception is years of schooling (and being behind grade for their age) of children in secondary school age, which tend to improve when exposed to oil price increases. However, the size of these effects is extremely small (0.00024 more years of education or 0.0053 percentage points less likely to be behind in school).

To further confirm the lack of an effect of higher oil prices on school enrollment, I repeat this exercise using the school enrollment data at the municipality-year level (as opposed to the education data at the individual level). For this matter, I estimate Equation 2 with primary enrollment rates as the outcome and present the results in Table 6. The table reveals that oil price shocks not only had small effects on school enrollment, but it further confirms the negative and significant effects on primary school enrollment. The estimated coefficient suggests that primary school enrollment decreased by 0.14 percentage points per year in the average oil municipality, when oil prices increased from 1998 to 2005[34]. There is no evidence of any effects of oil price shocks on secondary school enrollment. As Table 7 shows, the coefficient of the interaction between oil production and oil prices is negative, but statistically insignificant and very small, which is also consistent with the results at the individual level previously described.

Overall, this section has shown that oil price shocks had null effects on children education outcomes. Furthermore, the evidence points in the direction of having small, but surprisingly negative effects on primary school enrollment. This paradoxical result happened in the context of a rise in oil royalties that, according to the Colombian law, should have been invested in education. The following section explores the mechanisms behind this result and provides evidence suggesting violence as a possible explanation.

6. Mechanisms: Why is there a lack of an effect of oil price shocks on education?

[34] This number is calculated by transforming the estimated semi-elasticity into annual percentage changes in the average oil municipality, in the same manner that I interpreted the coefficients earlier in this section.

This section explores the mechanisms behind this paradoxical result, by estimating the effect of oil price shocks on different outcomes at the municipality level. The section shows that oil price shocks increased national money transfers to local governments (including oil royalties) and that these additional resources were reserved for and later invested in the social sector. The investments, however, were concentrated in the health sector, but not on the education sector. This section also provides evidence that paramilitary violence increased with the additional oil resources. Taken together, these two results suggest that the higher violence brought by oil price shocks may have undermined any effect of the additional social investment, if there was indeed such an effect.

6.1. Oil Price Shocks Increase Municipality Revenue and Expenditures

In this section, I present the results of estimating the effect of oil price shocks on public finances. The specification used includes the contemporaneous exposure to oil price shocks as in equation (2), plus an additional term for the lagged exposure of municipalities to such shocks. The lagged term is added because oil resources in a given year could have been reserved to be spent the following year in specific investment projects. The specification can be represented by the following equation

$$y_{mrt} = \beta_0 + \lambda_1(Oil_m \times OP_t) + \lambda_2(Oil_m \times OP_{t-1}) + Z_{mt}\beta_1 + \alpha_m + g_t + \mu_r(t) + u_{mrt} \quad (3)$$

where y_{mt} is public revenue or expenditures in municipality m in year t; Oil_m is the oil production level in municipality m during 1988; OP_t is the log of international price of oil in year t; α_m are municipality fixed effects; g_t are year fixed effects; Z_{mt} are time-varying municipality controls and $\mu_r(t)$ are region-specific linear time trends.

Columns 1 and 2 in Table 7 report the effect of contemporaneous and lagged oil price shocks on total revenue and expenditures. The table reveals that increases in oil prices generated higher municipality revenue, since the coefficient of the contemporaneous exposure to oil price shocks in Column 1, oilprod88xlop(t), is positive and significant.

Column 2 shows that the additional resources seem to have been spent in subsequent years, as the coefficient of oilprod88xlop(t-1) is significant and large, relative to the coefficient of oilprod88xlop(t), which is insignificant.

To confirm that the additional oil resources were reserved and spent in subsequent years, Columns 3 to 5 present the effect of oil price shocks on the fiscal superavit and on sources of funding. The sources of funding include debt and bequests, where bequests refer to funds from previous years that were reserved to be spent in the current fiscal year, in specific projects. Positive bequests denote money reserved from previous fiscal years to be spent today, while negative bequests denote money from the current fiscal year reserved to be spent in the future. The results are presented in such a way that the sum of columns 3 to 5 is zero (e.g. that the superavit is used to pay debt or to reserve money for future fiscal years).

The results in Columns 3 to 5 indicate that oil price shocks generated a superavit that was mostly reserved for future fiscal years. The coefficient of oilprod88xlop(t) for bequests is negative (Column 5), denoting funds being reserved for the future, and has almost the same magnitude as the coefficient for total superavit (Column 3). Some of the superavit was also used to pay debt, as the coefficient in Column 4 is negative and significant, although the coefficient is small relative to the coefficient of bequests. Consistent with these results, bequests this year are higher when there has been an oil price shock the year before, as the coefficient of oilprod88xlop(t-1) in Column 5 is positive and significant.

How much did revenue and expenditures increase with oil price shocks? To answer this question, Table 8 transforms the coefficients in Table 7 into annual percentage changes in the average oil municipality[35]. Panel A displays the interpreted coefficients for contemporaneous oil price shocks (e.g. oilprod88xlop(t) in Table 7), while Panel B presents the analog coefficients for lagged oil price shocks (e.g. oilprod88xlop (t-1)). In addition, Table 8 presents the effect of oil price shocks on additional specific revenue and expenditure categories of interest. The categories are government transfers to municipalities with mandatory destination (which include oil royalties) and investment, broken down into expenditures in physical assets (labeled fixed capital accumulation) and social investment (which includes teacher wages, school supplies, hospital expenditures, among other).[36]

The table reveals that contemporaneous oil price shocks increased total revenue by 4% and, in particular, government transfers with mandatory destination (which include oil royalties) by 5.8% per year in the average oil municipality. As described earlier, this increase in revenue did not translate into higher total expenditures until later years. Column 2 in Panel A shows that the contemporaneous response of expenditures and investment to oil price shocks was limited, since the coefficient of oilprod88xlop(t) is insignificant and small (the percent change ranges between 0.5% and 0.8% per year). Instead, the additional oil money seems to have been invested later as evidenced by the coefficient of lagged oil price shocks in Column 2, Panel B. In particular, the increase in oil prices generated a rise in investment the following year, of 2.5% and 4.5% in physical assets and in the social sector, respectively.

What was the money invested on? To answer this question, Table 9 reports the effect of oil price shocks on investment by sector.[37] It reveals that the additional oil resources are

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[35] The percentage changes are calculated by multiplying the estimated coefficients by the average oil production in oil municipalities (0.084 hundred thousand barrels/day), which yields the semi-elasticity of revenue/expenditures to oil prices in the average oil municipality. This semi-elasticity is then multiplied by the change in log oil prices between 1998 and 2005 (1.37), when oil prices tripled. The calculation gives the change in the dependent variable in an 8-year period. This number is then divided by 8 and by the average value of the dependent variable, yielding the average percentage change per year, in the average oil municipality.

[36] Appendix tables A.3 and A.4 present the effect of oil price shocks on other municipality revenue and expenditure categories.

[37] These categories of investment include both investment in physical assets and the social sector.

mostly invested in the health sector, as revealed by the contemporaneous and lagged coefficients in Column 1 of both Panels in the Table. In particular, contemporaneous increases in oil prices incremented health investment by 1.1%, while the lagged increases in prices boosted health expenditures by 2.7% (Column 2). Investment in education, on the other hand, increased by 3% one year after oil price shocks happened, as evidenced by Column 2 in Panel B. However, the estimate is imprecise and significant only at the 90% confidence level.[38]

Taken together, the results of this section suggest that the additional revenue generated by oil price shocks was reserved and invested during subsequent years. The larger resources were mainly invested in health, although there is some (imprecise) evidence of increased investment in the education sector as well. That lack of a (precise) effect of oil prices shocks on education investment might explain why the additional oil resources has no effects on children educational outcomes. However, it does not seem to be a satisfactory explanation for the small, but negative effects on primary school enrollment. The following section explores the role of violence in explaining this negative result.

6.2. Oil price Shocks Fuel the Civil Conflict, by Increasing Paramilitary Violence

This section provides evidence that suggests violence as a possible mechanism behind the small but negative effect of oil price shocks on primary school enrollment. To do so, the effect of oil price shocks on paramilitary violence is estimated, according to equation (2).

Table 11 presents the results of this estimation and reveals that oil price shocks increased paramilitary violence in oil municipalities. As column (3) in the table shows, a rise in oil prices increased paramilitary attacks in areas that produce more oil. This

[38] The evidence with regards to investment in the water sector is inconclusive, since contemporaneous oil price shocks increased investment in this sector by 7%, but lagged shocks decreased it by 4%. Investment in transportation, on the other hand, decreased both in response to contemporaneous and lagged oil price shocks. The coefficients of total investment in Table 9 do not match exactly the coefficients of total investment in Table 8 because the tables use different databases. However, both tables display consistent signs and similar magnitudes. See the data section for more details regarding the databases used.

result replicates Dube and Vargas (2013), who find that increases in oil prices fueled paramilitary violence in Colombia.

To assess the magnitude of this effect, I calculate the average annual change in paramilitary attacks due to oil price increases, and find that paramilitary violence increased 8% per year, due to the rise in oil prices between 1998 and 2005. This number is calculated as follows: oil prices rose by 1.37 log points between 1998 and 2005. In the average oil producing municipality, with production levels of 0.084 hundred thousand barrels per day, this price increase translates into 0.08 more attacks over this 8 year period, compared with a municipality without oil. This, in turn, corresponds to 0.01 more attacks per year, representing a 8% differential increase in paramilitary attacks above the mean (0.12).

These results suggest that paramilitary violence could explain the negative effect of oil price shocks on primary school enrollment, because higher exposure to a civil conflict has been shown to negatively affect educational attainment (León, 2012; Akresh and de Walque, 2010; Akbulut-Yuksel, 2008; Chamarbagwala and Morán, 2011; Duque, 2014; Sánchez and Dominguez, 2012). Nonetheless, child labor could also be a mechanism, a matter that is addressed in the next subsection.

6.3. The Effect of Oil Price Shocks on Local Labor Market Outcomes

The previous section provided evidence that oil price increases fueled the civil conflict, which could explain the small but negative effects of higher oil prices on primary school enrollment. This section explores whether the effects on local labor markets could also explain this result, by estimating how wages and hours worked responded to exposure to oil price shocks. If the oil price increase would raise product demand and hence associated labor demand, it could affect school enrollment in two ways. On one hand, an increase in household welfare could raise investment in education, improving school enrollment. On the other hand, if the higher labor demand would incentivize child labor, students would be induced to drop out of school, therefore decreasing school enrollment.

This mechanism is explored using a separate database, the rural component of the National Household Survey (ENH is its acronym in Spanish) a household survey conducted in 23 of the 32 Colombian departments. The outcomes analyzed are hours worked during the past month, and well as hourly wages for wage and salary workers. The sample consists of employed individuals on official working age in Colombia, i.e. those between ages 14 and 60. Although the sample analyzed are mainly adults (as opposed to children) the results are informative about effects on the labor market that could affect child labor.

Table 12 reports the effect of oil price shocks on hours worked and hourly wages. The results indicate that the effects are insignificant on both labor market outcomes. The coefficient for the number of hours worked (Column 2) is small, since it reveals that a 1% increase in oil prices increases hours worked by 0.004% in the average oil municipality. This represents a 0.2% increase in hours worked per year given the increase in oil prices between 1998 and 2005. The coefficient for hourly wages, on the other hand, is medium sized, since it indicates that a 1% increase in oil prices generated a rise of 0.1% in hourly wages. This means that the increase in oil prices between 1998 and 2005 increased wages by 3.9% per year.

The (imprecise) effect of oil price shocks on hourly wages appears to be non-negligible. However, there are three factors that should be taken into account when interpreting this result. The first one is that the oil sector in Colombia is non-labor intensive and it uses specialized machinery that requires a workforce with specific skills to operate them. Therefore, higher oil production is unlikely to generate more demand for low-skilled labor, which is the type of effect that could induce children out of school and into the workforce. Second, the estimate on hourly wages is not statistically different from zero. Third, this estimate seems to be small when compared to the effect of price shocks on commodities that are labor intensive. Indeed, as Dube and Vargas (2013) show, a 1% increase in the price of coffee (a labor intensive sector), increased wages by 0.5% and number of hours worked by 0.43%, an effect that is at least 5 times higher than the effect of oil price shocks. Taken together, these three factors seems to support the hypothesis that large effects on child labor are unlikely to explain the results.

8. Discussion and Conclusion

This paper investigates the effect of higher economic activity on educational outcomes in the context of Colombia, an oil producing country with a long-standing civil conflict. The higher economic activity studied takes the form of larger oil royalties, coming from increases in oil prices in international markets. The paper provides evidence showing that the increases in oil prices between 1998 and 2005 had limited effects on years of schooling or children's grade for age. Even more surprising, oil price shocks had small but negative effects on primary school enrollment. These are puzzling results, because the additional oil resources should be spent in education, among other social sectors, as dictated by the Colombian law. The results indicate that the additional oil resources were saved and later invested primarily in the health sector, with no evidence of significant additional investments in the education sector. Furthermore, the evidence indicates that instead of fueling education, the higher oil revenue fueled the Colombian civil conflict. Indeed, the increased oil monetary transfers gave incentives to illegal right wing paramilitary groups to steal oil resources, resulting in an 8% increase in paramilitary violence in oil producing municipalities.

The intensified paramilitary violence, which consisted mainly of civilian massacres, could have affected children's education in several ways. Massacres spread fear amongst the population creating stress and anxiety, which has been shown to affect students' behavior and performance (Sharkey et al., 2012, 2014; Bowen and Bowen 1999; Delaney-Black 2002; Hurt et al. 2001). Paramilitary violence also generated massive forced displacement of families. The families lost their income and assets and their children were forced to leave school (Uwaifo and Wharton, 2013). Even if the length of the school leave was short, the adjustment to new settings could have been challenging, disrupting children's school performance (Stermac et al., 2013). In addition, the quality of the education in high conflict regions might have decreased because high violent areas attract less qualified teachers, which affects the education of those children who stay in high conflict zones (Grogger, 1997; Sánchez and Rodríguez, 2010).

The evidence presented here reveals that the civil conflict might be an alternative explanation for the paradoxical negative effect of higher economic activity on primary education. Existing studies in civil-war free contexts have ascribed a bigger role to child labor, because higher local economic activity might increase the wages of low-skilled workers, inducing children into employment and out of school. Some examples are Kruger (2007) and Duryea and Arends-Kuenning (2003), who study coffee price shocks and local labor market conditions in Brazil; or Black, McKinnish and Sanders (2005) and Cascio and Narayan (2015), who study coal price shocks or oil and gas extraction shocks in the US. Yet, when the higher economic activity happens in the context of a civil conflict, increased violence emerges as an alternative explanation for the paradoxical results.

Indeed, this paper shows that the civil conflict is a more likely explanation that child labor, in the context of the Colombian oil sector. Although wages seem to have increased with oil prices, the increase is imprecisely estimated and substantively small, when compared to the wage increase generated by price shocks in commodities that, unlike oil, are labor intensive. Indeed, the estimates here presented are at least five times lower than the effect of higher coffee prices (a labor intensive sector), estimated by Dube and Vargas (2013).

This paper has important policy implications for countries rich in natural resources (legally or illegally extracted), that are embedded in civil conflicts. The findings suggest that these countries could be harmed in the long run by economic shocks that favor them in the short run. Such is the case of Iran, Libya, Iraq or Qatar, which are big oil exporters and also have been affected by short or long term civil wars. Or the case of countries that produce or transport illegal drugs, since the profits from surges in illegal drug prices rarely benefit local residents and instead bring high levels of drug-cartel related violence (Angrist and Kugler, 2008). Such is the case of México, Guatemala, El

Salvador and Honduras. Education and welfare programs in these type of countries should target and monitor children who live or have lived in high conflict zones, to prevent losing valuable human capital for the long run development of countries.

9. References

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10. Figures and Tables

Figure 1. Paramilitary attacks in oil municipalities increase with oil prices

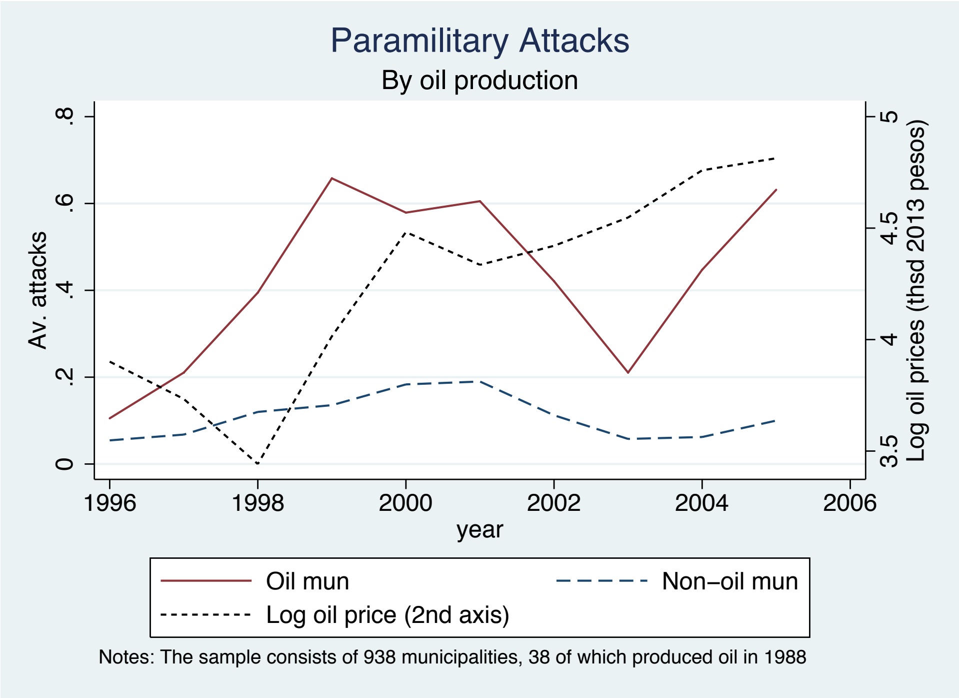

Figure 2. Paramilitary attacks increase after the paramilitary coalition in 1997

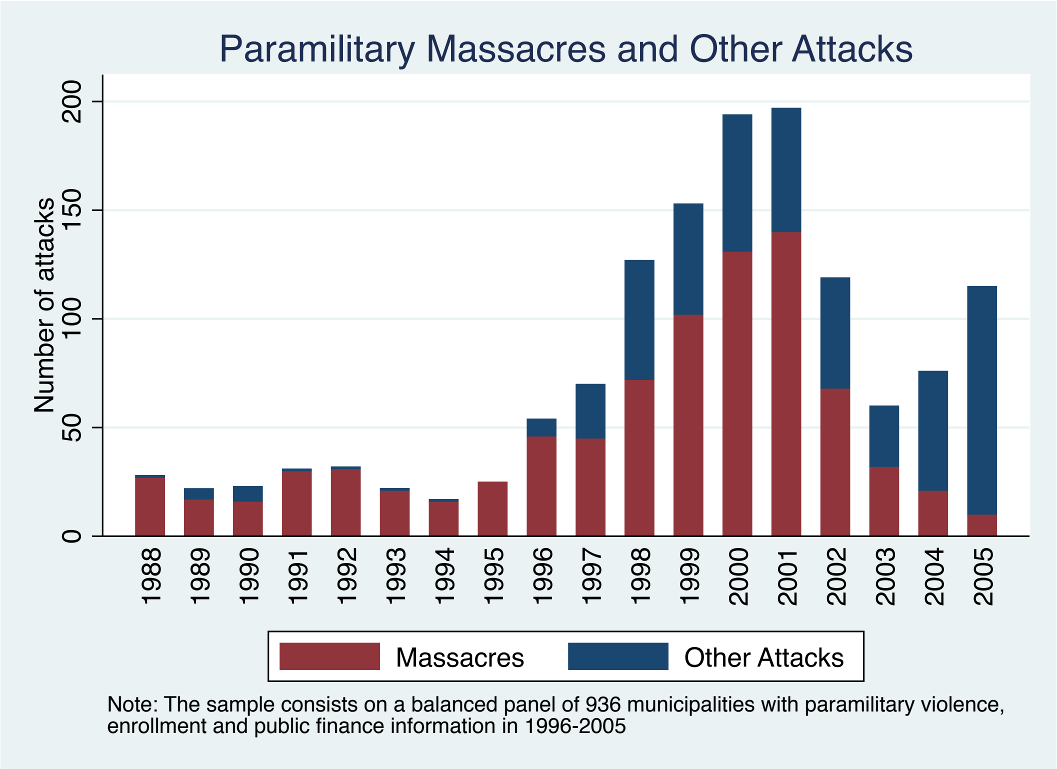

Figure 3. Paramilitary violence is also reflected in homicides

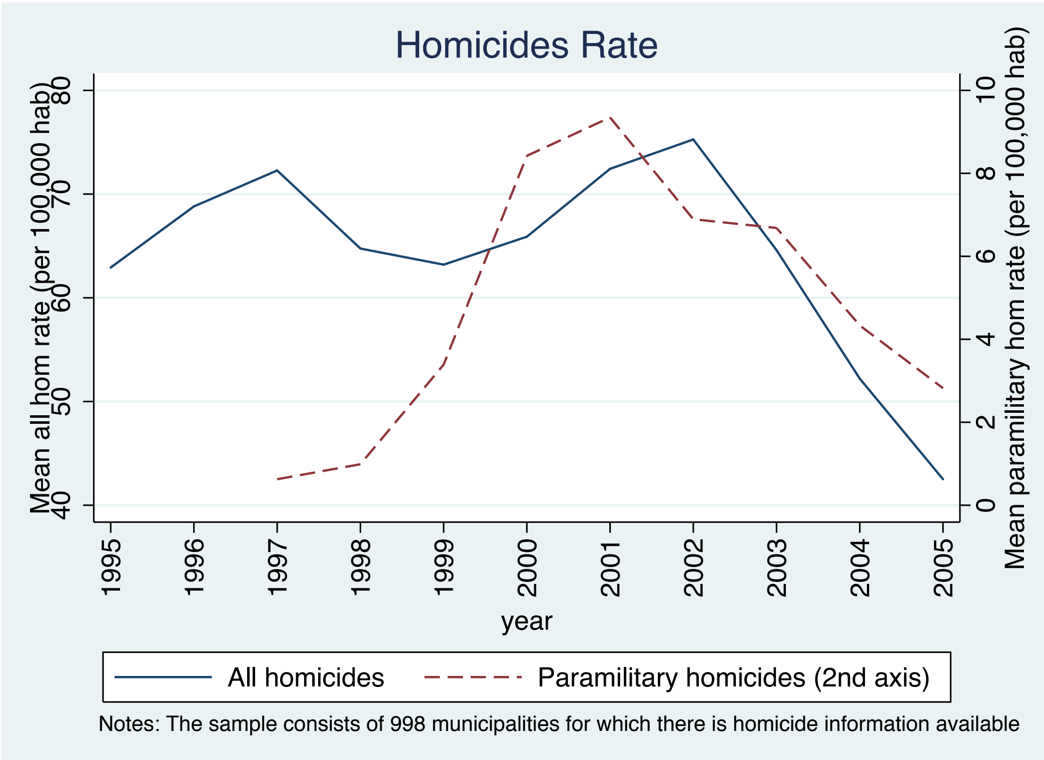

Figure 4. National transfers increase with oil prices in oil municipalities

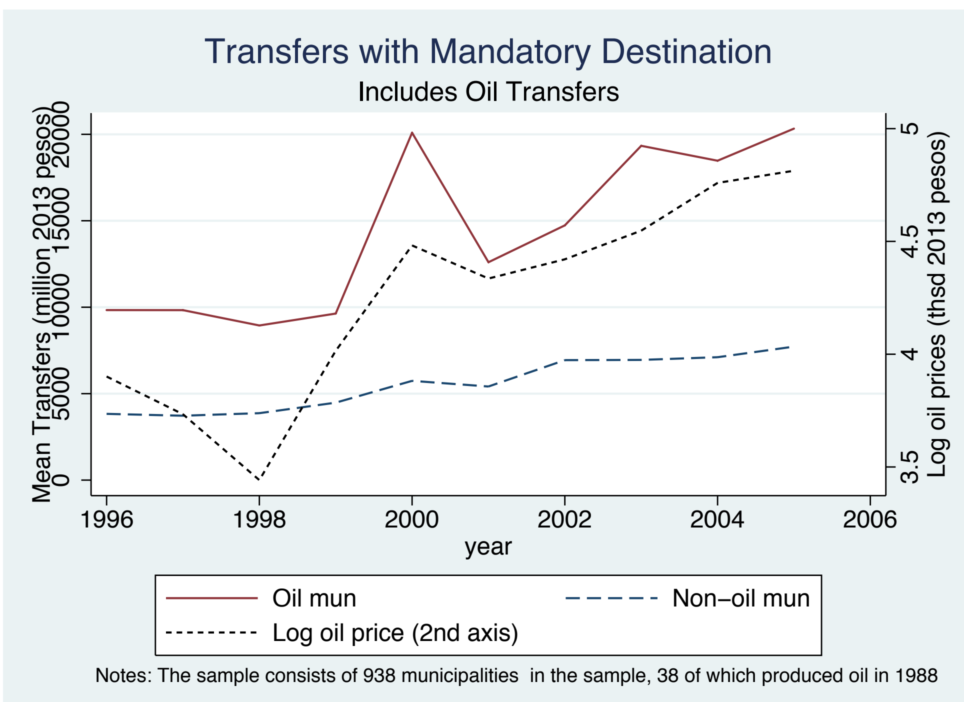

Figure 5. Social Expenditures do not seem to increase differentially in oil municipalities

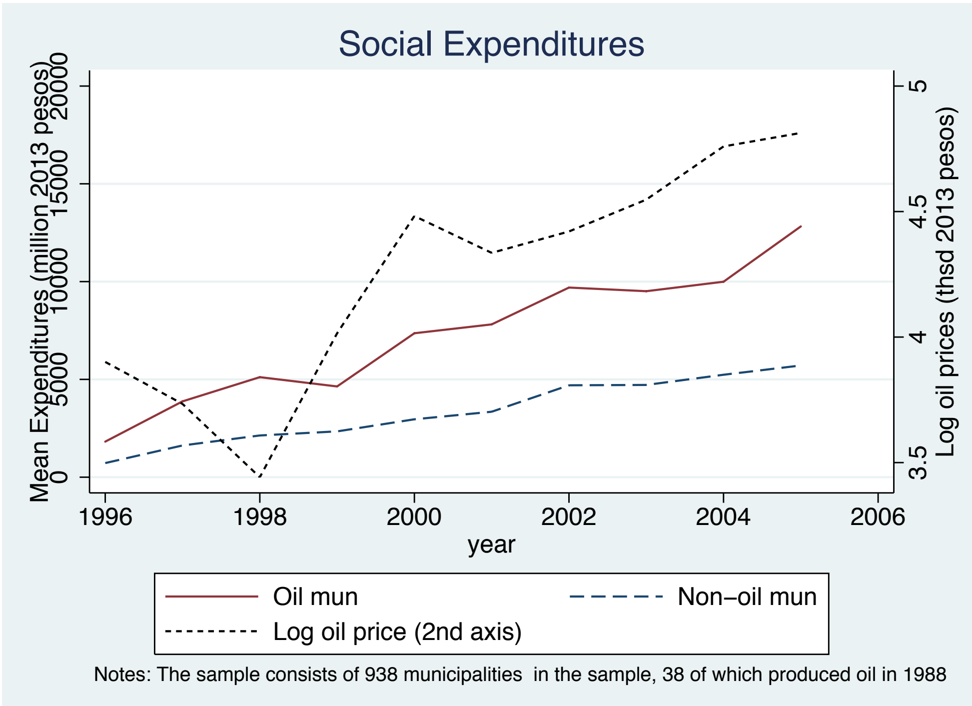

Figure 6. The lifetime effect of oil price shocks on school enrollment

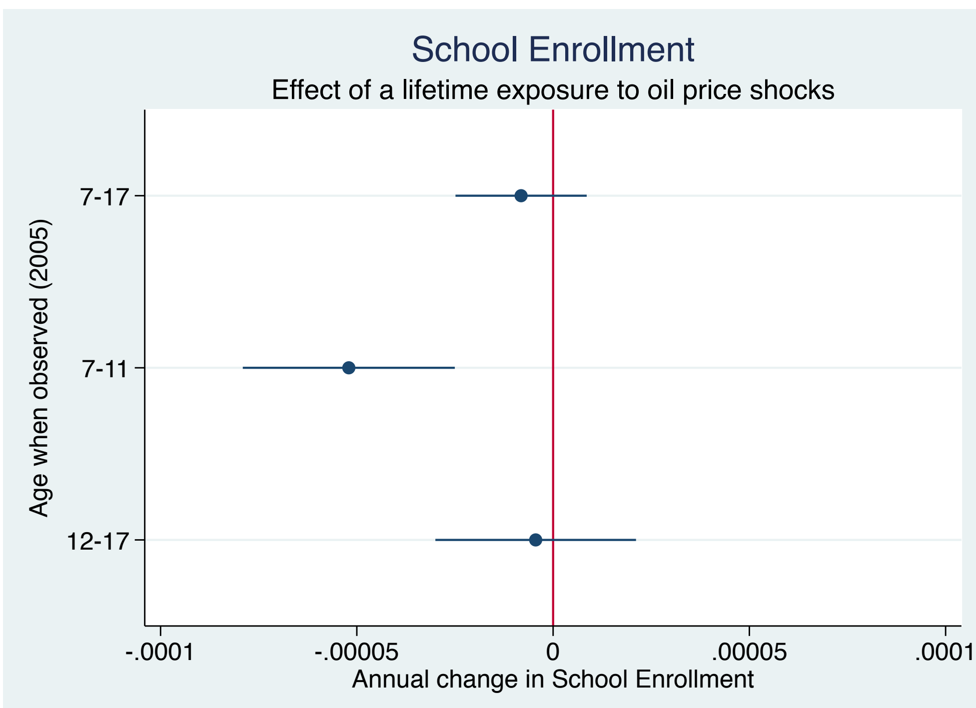

Figure 7. The lifetime effect of oil price shocks on whether the child is behind grade

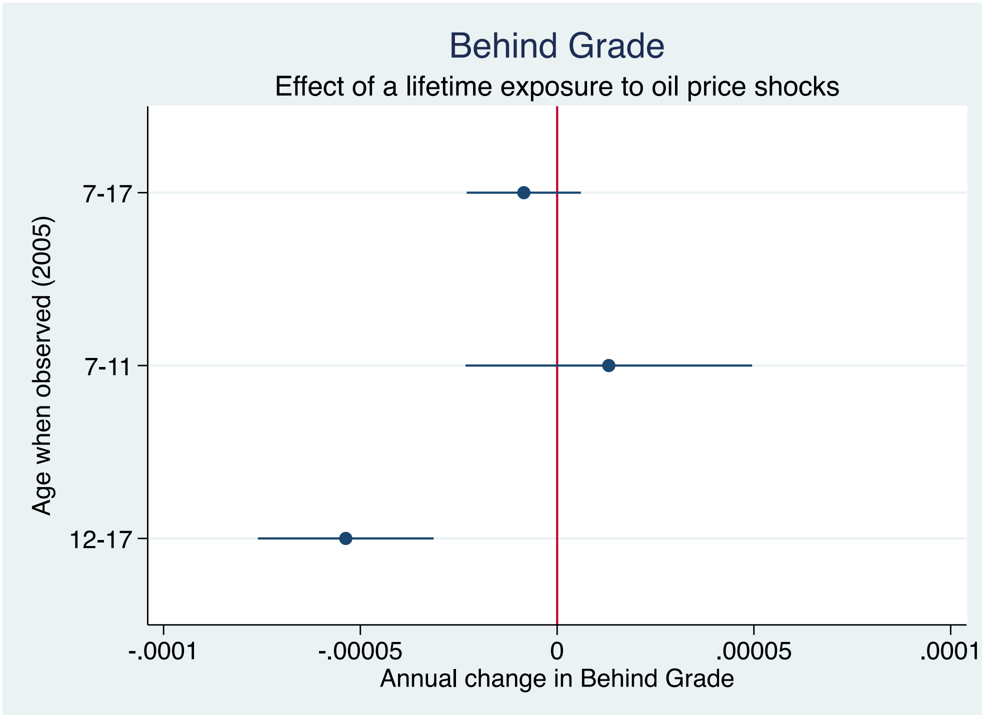

Figure 8. The lifetime effect of oil price shocks on years of schooling

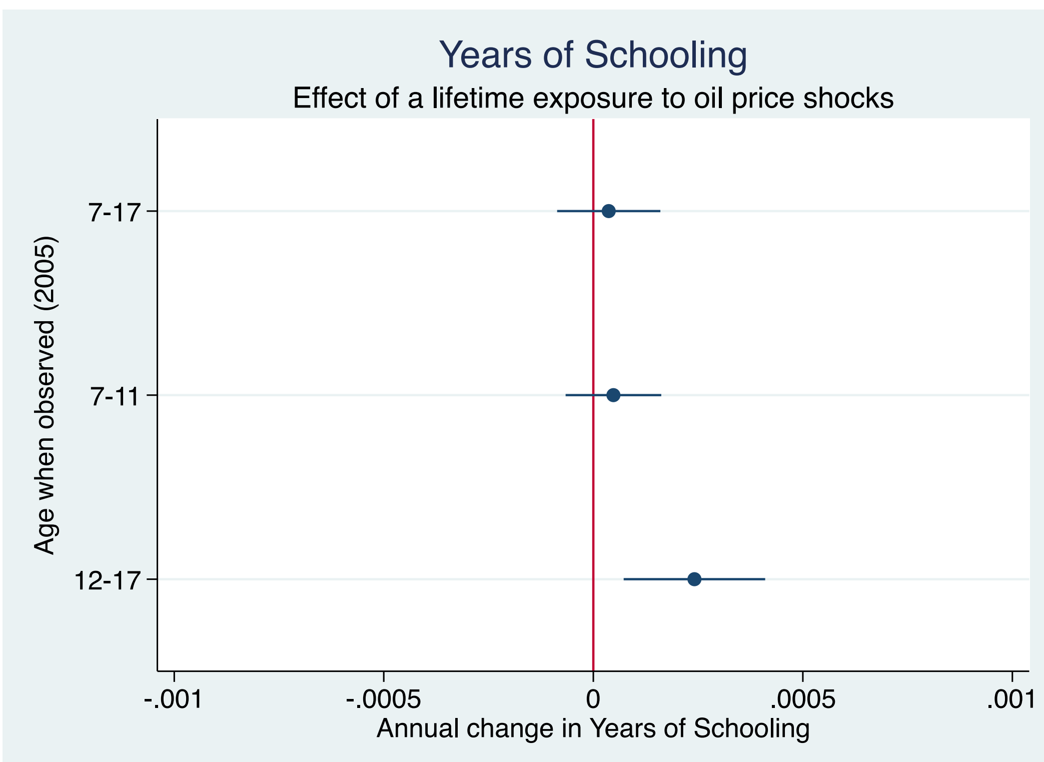

Table 1. Summary Statistics of Children, by Oil Production

VARIABLES	Oil mun (2)	Non-oil mun	Diff
	(1)	(2)	(3)
N. children	41,678	589,607	
N. (grouped) municipalities	35	461	
Attends school	0.795	0.805	-0.01***
	(0.404)	(0.396)	
Behind grade for age (1)	0.360	0.354	0.01**
	(0.480)	(0.478)	
N. years schooling	4.581	4.589	-0.01
	(2.805)	(2.890)	
Ever exposed to a paramilitary attack (3)	0.746	0.582	0.16***
	(0.435)	(0.493)	
N. paramilitary attacks exposed to (4)	9.580	3.791	5.79***
	(14.706)	(6.537)	
Primary school age (7-11)	0.465	0.460	0.00*
	(0.499)	(0.498)	
White	0.875	0.801	0.07***
	(0.331)	(0.400)	
Black	0.065	0.110	-0.05***
	(0.246)	(0.313)	
Indigenous	0.054	0.083	-0.03***
	(0.227)	(0.276)	
Female	0.489	0.485	0.00*
	(0.500)	(0.500)	
Head less than primary education	0.554	0.553	0.00
	(0.497)	(0.497)	
Head primary educatrion	0.329	0.324	0.00*
	(0.470)	(0.468)	
Head secondary education	0.095	0.099	-0.00***
	(0.293)	(0.299)	
Head female	0.244	0.238	0.01***
	(0.430)	(0.426)	
Household size	5.719	5.795	-0.08***
	(2.328)	(2.315)	
Lives in urban area	0.570	0.498	0.07***
	(0.495)	(0.500)	

Notes: Children in sample consist of individuals ages 7-17 in 2005 with non-missing control characteristics and who were born in municipalities with armed conflict information. (1) Behind grade takes 1 if the age minus the years of schooling of the child is 8 or more. (2) An oil municipality is a municipality that produced oil in 1988. (3) Ever exposed if the municipality where the child was born experienced a paramilitary attack during the child's life. (4) N. paramilitary attacks the municipality of birth experienced during the child's life

Table 2. Summary Statistics of municipalities, by Oil Production

VARIABLES	All	Oil mun	Non-oil mur
Number of municipalities	936	38	898
Number of municipality x year observations	9360	380	8980
Municipality-year level variables			
Paramilitary attacks	0.12	0.65	0.11
	(0.52)	(1.13)	(0.47)
Paramilitary massacres	0.07	0.46	0.06
	(0.37)	(0.73)	(0.34)
Enrollment rate	0.89	0.95	0.89
	(0.19)	(0.14)	(0.19)
Primary enrollment rate (1)	1.23	1.27	1.23
	(0.25)	(0.22)	(0.25)
Secondary enrollment rate (2)	0.58	0.54	0.58
	(0.24)	(0.17)	(0.24)
Population - Million habitants	0.02	0.04	0.02
	(0.03)	(0.04)	(0.03)
Public transfers w/ mandatory destination	5,909	14,381	5,551
(includes oil revenue) - 2013 million pesos (4)	(8,254)	(16,134)	(7,540)
Public social expenditures - 2013 million pesos	3,499	7,264	3,339
	(6,172)	(11,331)	(5,802)
Municipality level variables			
Ever had a paramilitary attack 1996-2005	0.36	0.66	0.35
	(0.48)	(0.48)	(0.48)
Total paramilitary attacks 1996-2005	1.21	4.26	1.09
	(2.82)	(7.02)	(2.42)
Ever had a paramilitary massacre 1996-2005	0.27	0.45	0.26
	(0.44)	(0.50)	(0.44)
Total paramilitary massacres 1996-2005	0.69	2.11	0.63
	(1.81)	(4.34)	(1.60)
Pct urban population 2005	0.41	0.50	0.41
	(0.23)	(0.20)	(0.23)
NBI - Poverty Index 2005	52.55	56.13	52.40
	(18.37)	(14.41)	(18.51)
Produced oil in 1988	0.04	1.00	0.00
	(0.20)	0.00	0.00
Oil production in 1988 - hundred thsd barrels/c	0.00	0.08	0.00
	(0.05)	(0.26)	0.00
Year level variables			
Oil price -Thsd 2013 pesos/barrel	76.01	76.01	76.01
	(31.12)	(31.12)	(31.12)

Notes: The sample consists of a balanced panel of municipalities with information on school enrollment, paramilitary violence and other covariates for the period 1996-2005. (1) Primary enrollment defined as the N. students enrolled in primary as a pct of the N children 7-11 years old. (2) Secondary enrollment defined as the N. students enrolled in secondary as a pct of the N children 12-17 years old. (3) This poverty index denotes the percentage of households in the municipality with their basic needs unsatisfied. (4) Denotes transfers from the national government to the municipality, that must be invested in specific sectors; includes transfers for the extraction of natural resources.

Table 3. The limited effect of oil price shocks on school enrollment

Dependent variable: Enrolled in school

	Sample: Children age in 2005					
	7-17		7-11 (primary school)		12-17 (secondary school)	
Cumulative effect throughout	(1)	(2)	(3)	(4)	(5)	(6)
Lifetime	-0.000328		-0.00210***		-0.000179	
	(0.000)		(0.001)		(0.001)	
Ages 0-6		-0.0318***		0.0748		0.0303
		(0.007)		(0.046)		(0.037)
Ages 7-11		-0.00761***		0.00804		-0.0197
		(0.002)		(0.008)		(0.030)
Ages 12-17		0.00194***				-0.00759
		(0.001)				(0.005)
Observations	631,285	631,285	290,416	290,416	340,869	340,869
R-squared	0.200	0.200	0.101	0.101	0.187	0.187
N. (grouped) municipalities	496	496	496	496	496	496

Note: Standard errors clustered at the municipality level. The sample consists of children ages 7-17 in 2005, who were born in municipalities with oil production information available. The cumulative effect of oil price shocks is based on the estimated coefficient of oilprod88xlop at age k, which consists of the interaction of oil production in the municipality of birth (as of 1988) with log oil price when the child was age k. If the child had not achieved age k in 2005, this term takes zero. The term cumulative oilprod88xlop from ages i to j, consists of the sum of oilprod88xlop from age i to age j. The term cumulative oilprod88xlop during lifetime consists of the sum of oiprod88xlop over the child's life. The regressions include child level controls, municipality and year fixed effects, time-varying municipality controls and region-specific linear time trends. Child level controls consist of gender, race/ethnicity indicators, head of the household education level (less than primary, primary, secondary), head of the household gender, household size and whether the household lives in an urban area. Municipality log-population by year included in the regressions. Region-specific linear time trends for each geographic region in Colombia (four in total) included, and for coca producing and non-producing census-units. Municipalities with less than 20,000 habitants are grouped with neighboring municipalities in the 2005 census, forming census-units. There are a total of 538 census units, of which 496 are included in these regressions.

Table 4. The limited effect of oil price shocks on whether the child is behind grade

Dependent variable: Behind grade

	Sample: Children age in 2005					
	7-17		7-11 (primary school)		12-17 (secondary school)	
Cumulative effect throughout	(1)	(2)	(3)	(4)	(5)	(6)
Lifetime	-0.0007		0.0011		-0.00433***	
	(0.0006)		(0.0015)		(0.0009)	
Ages 0-6		0.00889		0.0773**		0.108**
		(0.008)		(0.038)		(0.049)
Ages 7-11		0.00163		0.0129**		-0.0422
		(0.002)		(0.006)		(0.029)
Ages 12-17		-0.00154**				-0.0253***
		(0.001)				(0.004)
Observations	631,285	631,285	290,416	290,416	340,869	340,869
R-squared	0.280	0.280	0.176	0.176	0.203	0.203
N. (grouped) municipalities	496	496	496	496	496	496

Note: Standard errors clustered at the municipality level. The sample consists of children ages 7-17 in 2005, who were born in municipalities with oil production information available. The cumulative effect of oil price shocks is based on the estimated coefficient of oilprod88xlop at age *k,* which consists of the interaction of oil production in the municipality of birth (as of 1988) with log oil price when the child was age *k*. If the child had not achieved age *k* in 2005, this term takes zero. The term cumulative oilprod88xlop from ages *i* to *j*, consists of the sum of oilprod88xlop from age *i* to age *j*. The term cumulative oilprod88xlop during lifetime consists of the sum of oiprod88xlop over the child's life. The regressions include child level controls, municipality and year fixed effects, time-varying municipality controls and region-specific linear time trends. Child level controls consist of gender, race/ethnicity indicators, head of the household education level (less than primary, primary, secondary), head of the household gender, household size and whether the household lives in an urban area. Municipality log-population by year included in the regressions. Region-specific linear time trends for each geographic region in Colombia (four in total) included, and for coca producing and non-producing census-units. Municipalities with less than 20,000 habitants are grouped with neighboring municipalities in the 2005 census, forming census-units. There are a total of 538 census units, of which 496 are included in these regressions.

Table 5. The small effect of oil price shocks on years of schooling

Dependent variable: Years of schooling

	Sample: Children age in 2005					
	7-17		7-11		12-17	
Cumulative effect throughout	(1)	(2)	(3)	(4)	(5)	(6)
Lifetime	0.00297		0.00387		0.0194***	
	(0.005)		(0.005)		(0.007)	
Ages 0-6		0.146***		0.478***		-0.193
		(0.023)		(0.154)		(0.270)
Ages 7-11		0.0195***		0.0772***		0.586**
		(0.007)		(0.024)		(0.265)
Ages 12-17		-0.00316				0.141***
		(0.005)				(0.027)
Observations	631,285	631,285	290,416	290,416	340,869	340,869
R-squared	0.589	0.589	0.491	0.491	0.317	0.317
N. (grouped) municipalities	496	496	496	496	496	496

Note: Standard errors clustered at the municipality level. The sample consists of children ages 7-17 in 2005, who were born in municipalities with oil production information available. The cumulative effect of oil price shocks is based on the estimated coefficient of oilprod88xlop at age k, which consists of the interaction of oil production in the municipality of birth (as of 1988) with log oil price when the child was age k. If the child had not achieved age k in 2005, this term takes zero. The term cumulative oilprod88xlop from ages i to j, consists of the sum of oilprod88xlop from age i to age j. The term cumulative oilprod88xlop during lifetime consists of the sum of oiprod88xlop over the child's life. The regressions include child level controls, municipality and year fixed effects, time-varying municipality controls and region-specific linear time trends. Child level controls consist of gender, race/ethnicity indicators, head of the household education level (less than primary, primary, secondary), head of the household gender, household size and whether the household lives in an urban area. Municipality log-population by year included in the regressions. Region-specific linear time trends for each geographic region in Colombia (four in total) included, and for coca producing and non-producing census-units. Municipalities with less than 20,000 habitants are grouped with neighboring municipalities in the 2005 census, forming census-units. There are a total of 538 census units, of which 496 are included in these regressions.

*** $p<0.01$, ** $p<0.05$, * $p<0.1$

Table 6. The small but negative effect of oil price shocks on primary school enrollment at the municipality level

Dependent variable: Enrollment rate in primary school

	OLS	FE	Linear Trends
	(1)	(2)	(3)
oilprod88xlop	-0.127***	-0.127***	-0.0729**
	(0.036)	(0.038)	(0.035)
oilprod88	0.496***		
	(0.159)		
lop	-0.0328***		
	(0.007)		
Annual % change 98-05	-0.0024	-0.0024	-0.0014
Observations	9,360	9,360	9,360
R-squared	0.003	0.689	0.708
N. mun	936	936	936
Mun and year FE	N	Y	Y
Mun*year controls and region linear trends	N	N	Y

Note: The sample consists on a balanced panel of municipalities with information on school enrollment, paramilitary violence and other covariates for the period 1996-2005. Municipality*year controls consist of log population, while region linear trends consist of linear trends for each geographic region in Colombia (four in total), and for coca producing and non-producing municipalities. Annual % change 98-05=oilprod88xlop*avoilprod*dop/8, where avoilprod is the average oil production in oil municipalities (0.084 hundred thsd barrels/day), and dop is the change in log oil prices between 1998 and 2005 (1.37 log points). It represents the average percentage change in the dependent variable (per year), due to the increase in oil prices between 1998 and 2005.

*** p<0.01, ** p<0.05, * p<0.1

Table 7. The effect of oil price shocks on secondary school enrollment at the municipality level

Dependent variable: Enrollment rate in secondary school

	OLS	FE	Linear Trends
	(1)	(2)	(3)
oilprod88xlop	-0.0711***	-0.0711***	-0.0208
	(0.017)	(0.018)	(0.030)
oilprod88	0.341***		
	(0.090)		
lop	0.0451***		
	(0.005)		
Annual % change 98-05	-0.0013	-0.0013	-0.0004
Observations	9,360	9,360	9,360
R-squared	0.007	0.826	0.835
N. mun	936	936	936
Mun and year FE	N	Y	Y
Mun*year controls and region linear trends	N	N	Y

Note: The sample consists on a balanced panel of municipalities with information on school enrollment, paramilitary violence and other covariates for the period 1996-2005. Municipality*year controls consist of log population, while region linear trends consist of linear trends for each geographic region in Colombia (four in total), and for coca producing and non-producing municipalities. Annual % change 98-05=oilprod88xlop*avoilprod*dop/8, where avoilprod is the average oil production in oil municipalities (0.084 hundred thsd barrels/day), and dop is the change in log oil prices between 1998 and 2005 (1.37 log points). It represents the average percentage change in the dependent variable (per year), due to the increase in oil prices between 1998 and 2005.

*** p<0.01, ** p<0.05, * p<0.1

Table 8. Municipality revenue increased with oil price shocks and was later invested

Dependent variable (Million 2013 pesos)	Total Revenue	Total Expenditures	Total Superavit (-Deficit)	Funding	
				Debt	Bequests
	(1)	(2)	(3)	(4)	(5)
oilprod88xlop(t)	23,908**	-2952	26,860***	-3,170**	-23,690***
	(9947)	(7866)	(2982)	(1371)	(2666)
oilprod88xlop(t-1)	-8,291***	6,929***	-15,220***	557	14,662***
	(1882)	(1573)	(2973)	(1528)	(3160)
N. observations	9360	9360	9360	9360	9360
Rsquared	0.84	0.59	0.12	0.10	0.11

Notes: The sample consists of a balanced panel of municipalities with information on school enrollment, paramilitary violence and other covariates for the period 1996-2005. Standard errors in parenthesis are clustered at the municipality level. The regression includes municipality and year fixed effects; linear trends by geographic region (4 in total); linear trends for coca producing and non-producing municipalities; and log population.

Table 9. The size of the effect of oil price shocks on revenue and expenditures

Panel A. Effect of contemporaneous oil price shocks

Dependent variable (Million 2013 pesos)	Coefficient of oilprod88xlop (t) (1)	Annual % change dep. (2)	Pct Total Revenue (3)	Pct Total Expenditures (4)
Total Revenue	23,908**	.040**	100%	
	(9,946)	(0.016)		
Gov. transfers w/ mandatory destination (include oil revenue)	23,837***	.058***	77%	
	(5,986)	(0.014)		
Total Expenditures	-2,952	-.004		100%
	(7,866)	(0.012)		
Investment	-3,206	-.007		76%
	(5,974)	(0.013)		
Fixed capital accumulation	-1,083	-.005		38%
	(3,104)	(0.015)		
Social investment	-2,123	-.008		38%
	(2,922)	(0.012)		

Panel B. Effect of lagged oil price shocks

Dependent variable (Million 2013 pesos)	Coefficient of oilprod88xlop (1)	Annual % change dep. (2)	Pct Total Revenue (3)	Pct Total Expenditure (4)
Total Revenue	-8,290***	-.013***	100%	
	(1,881)	(0.003)		
Gov. transfers w/ mandatory destination (include oil revenue)	-11,273***	-.027***	77%	
	(2225)	(0.005)		
Total Expenditures	6,928***	.011***		100%
	(1,573)	(0.002)		
Investment	16,177***	.036***		76%
	(3,139)	(0.007)		
Fixed capital accumulation	5,093***	.025***		38%
	(946)	(0.004)		
Social investment	11,084***	.045***		38%
	(3,149)	(0.013)		

Notes: Each row represents a different regression. The sample consists of a balanced panel of municipalities with information on school enrollment, paramilitary violence and other covariates for the period 1996-2005. Standard errors in parenthesis are clustered at the municipality level. The regression includes municipality and year fixed effects; linear trends by geographic region (4 in total); linear trends for coca producing and non-producing municipalities; and log population. Panel A presents the results for contemporaneous oil price shocks, while Panel B presents results for lagged oil price shocks. Column (1) reports the coefficient of the oil interaction term. Column (2)=(1)*avoilprod*dop/8, where avoilprod is the average oil production in oil municipalities (0.084 hundred thsd barrels/day), dop is the change in log oil prices between 1998 and 2005 (1.37 log points). It represents the average percentage change in the dependent variable (per year) for the average oil municipality, due to increases in oil prices, assuming an increase in oil prices similar to the one between 1998 and 2005. Columns (3) and (4) displays the corresponding dependent variable as a percentage of total revenue (expenditures), for the average oil municipality.

Table 10. Oil price shocks increased investment in health, but seemingly not in education

Dependent variable (Million 2013 pesos)	Panel A. Effect of contemporaneous oil price: Coefficient of oilprod88xlop (t)	Annual % change dep. var	Pct Tot Investment
	(1)	(2)	(3)
Total investment	4,221	.008	100%
	(5,834)	(0.011)	
Education	-878	-.008	20%
	(1,382)	(0.012)	
Health	1,617***	.011***	26%
	(278)	(0.001)	
Water and sewage	5,529***	.069***	18%
	(1,021)	(0.012)	
Transportation	-1,628***	-.026***	12%
	(314)	(0.005)	

Dependent variable (Million 2013 pesos)	Panel B. Effect of lagged oil price shocks: Coefficient of oilprod88xlop (t-1)	Annual % change dep. var	Pct Tot Investment
	(1)	(2)	(3)
Total investment	7,936***	.015***	100%
		(0.005)	
Education	3,326*	.030*	20%
		(0.018)	
Health	3,942***	.027***	26%
		(0.008)	
Water and sewage	-3,037**	-.038**	18%
		(0.016)	
Transportation	-1,379***	-.022***	12%
		(0.005)	

Notes: Refer to notes on Table 9.

Table 11. Oil price shocks increased paramilitary violence in the oil region

Dependent variable: Paramilitary attacks

	OLS	FE	Linear trends
Dependent variable	(1)	(2)	(3)
oilprod88 x log(oil price)	0.830***	0.833*	0.720***
	(0.108)	(0.483)	(0.119)
oilprod88	-2.967***		
	(0.224)		
log(oil price)	0.00685		
	(0.011)		
Av annual % change	0.099	0.099	0.086
Observations	9,360	9,360	9,360
Mun and year FE	N	Y	Y
Mun*year controls and region linear trends	N	N	Y

Notes: The sample consists of a balanced panel of municipalities with information on school enrollment, paramilitary violence and other covariates for the period 1996-2005. Standard errors in parenthesis are clustered at the municipality level. Municipality*year controls consist of log population. Region linear trends refer to linear time trends specific to the four major geographic regions in Colombia and linear trends for coca producing and non-producing municipalities. The av. annual % change corresponds to the percent change in paramilitary attacks per year, due to the increase in oil prices between 1998 and 2005. This number is calculated by multiplying the estimated coefficient with the average oil production in oil municipalities (0.084 hundred thsd barrels/day), the change in log oil prices between 1998 and 2005 (1.37 log points) and then dividing this number by the number of years in this period (8) and the average paramilitary attacks per year (0.12).

Table 12. The Effect of Oil Price Shocks on Local Labor Markets

Period: 1998-2005		
	Log wage	Log hours
Dependent variable	(1)	(2)
oilprod88 x lop	1.254	0.049
	(0.902)	(0.310)
Elasticity av. oil mun	0.106	0.004
Annual % change	0.039	0.002
95% CI	[-0.007 , 0.044]	[-0.008 , 0.010]
Indiv*year obs.	25,405	55,836
R-squared	0.117	0.124
N. mun	240	240

Note: Standard errors clustered at the municipality level. The regression includes municipality and year fixed effects; linear trends by geographic region (4 in total); linear trends for coca producing and non-producing municipalities; and log population. The av. annual % change corresponds to the percent change in paramilitary attacks per year, due to the increase in oil prices between 1998 and 2005. This number is calculated by multiplying the estimated coefficient with the average oil production in oil municipalities (0.084 hundred thsd barrels/day), the change in log oil prices between 1998 and 2005 (1.37 log points) and then dividing this number by the number of years in this period (8).

*** $p<0.01$, ** $p<0.05$, * $p<0.1$

11. Appendix

Table 13. Children in sample are representative of rural and small urban areas

	In sample		Out sample			
			No violence info		ncompl covariate	
VARIABLES	Mean	N	Mean	N	Mean	N
Attends school	0.80	631,285	0.90	197,405	0.73	44,412
Behind grade for age (1)	0.35	631,285	0.21	198,267	0.50	54,433
N. years schooling	4.59	631,285	5.08	197,507	4.02	47,530
Born in an oil municipality (2)	0.07	631,285	-	0	0.12	54,433
Ever exposed to a paramilitary attack	0.59	631,285	-	0	0.70	54,433
N. paramilitary attacks exposed to (4)	4.17	631,285	-	0	5.87	54,433
Primary school age (7-11)	0.46	631,285	0.48	198,267	0.49	54,433
White	0.81	631,285	0.88	198,267	0.51	54,433
Black	0.11	631,285	0.09	198,267	0.15	54,433
Indigenous	0.08	631,285	0.02	198,267	0.15	54,433
Female	0.48	631,285	0.49	198,267	0.48	54,433
Head less than primary education	0.55	631,285	0.25	182,039	0.75	5,894
Head primary educatrion	0.32	631,285	0.41	182,039	0.19	5,894
Head secondary education	0.10	631,285	0.23	182,039	0.05	5,894
Head female	0.24	631,285	0.27	198,267	0.08	54,433
Household size	5.79	631,285	4.83	198,267	4.88	54,433
Lives urban area	0.50	631,285	0.82	198,267	0.32	54,433

Notes: Children in sample consist of individuals ages 7-17 in 2005. Children out sample consist of children with i) incomplete information on control characteristics (covariates) or ii) children with no violence information. (1) Behind grade takes 1 if the age minus the years of schooling of the child is 8 or more. (2) An oil municipality is a municipality that produced oil in 1988. (3) Ever exposed if the municipality where the child was born experienced a paramilitary attack during the child's life. (4) N. paramilitary attacks the municipality of birth experienced during the child's life

Table 14. Municipalities in sample are representative of medium rural and small urban areas

	In sample		Out sample			
			Incompl enroll		No violence info	
VARIABLES	Mean	N	Mean	N	Mean	N
Municipality level variables						
Ever had a paramilitary attack 96-05	0.36	936	0.24	62	-	-
Total paramilitary attacks 96-05	1.21	936	0.45	62	-	-
Produced oil 1988	0.04	936	0.02	62	-	-
Pct urban population 2005	0.41	936	0.25	62	0.49	92
Poverty index 2005	52.6	925	67.6	61	55.2	58
Area (m2)	766	936	4,870	62	1,302	92
Distance to capital (km)	126	925	196	61	115	58
Altitude (m)	1,220	925	858	61	699	58
Rainfall (mm)	1,863	925	2,228	61	2,045	58
Municipality-year level variables						
Paramilitary attacks	0.12	9,360	0.05	620	-	-
Population - Hundred habitants	21,812	9,171	7,497	620	202,493	902
Tax revenue 1996-2005	1,566	9,360	289	464	49,571	884
Coca production 2001-2006	0.08	4,680	0.25	310	0.10	655
Land gini 2000-2006	0.69	4,835	0.65	258	0.67	458
Primary enrollment rate (1)	1.23	9,360	1.12	551	1.04	752
Secondary enrollment rate (2)	0.58	9,360	0.40	457	0.52	697

Notes: Municipalities in sample consist of those with paramilitary violence information and complete school enrollment information. (1) Primary enrollment is measured as the number of children enrolled in primary divided by the number of children 7-11 in the municipality. (2) Secondary enrollment is measured as the number of children enrolled in secondary divided by the number of children 12-17 in the municipality.

Table 15. Effect of oil price shocks on municipality revenue

Dependent variable (Million 2013 pesos)	Effect of contemporaneous oil price: Coefficient of oilprod88xlop (t)	Effect of contemporaneous oil price: Annual % change dep. var	Pct Tot revenue
	(1)	(2)	(3)
Total Revenue (1)+(2) +(3)	23,908**	.040**	100%
	(9,946)	(0.016)	
Municipality revenue (1)	1,057	.007	15%
	(4,445)	(0.031)	
Tax revenue	2,289	.021	11%
	(4,075)	(0.037)	
Non-tax revenue	-1,231***	-.036***	5%
	(424)	(0.012)	
Department transfers (2)	195***	.049***	0%
	(34)	(0.008)	
National transfers (3)	22,654***	.050***	85%
	(5,931)	(0.013)	
Discretionary Transfers	-1,182***	-.026***	8%
	(106)	(0.002)	
Transfers with Mandatory destination (include oil revenue)	23,837***	.058***	77%
	(5,986)	(0.014)	

Dependent variable (Million 2013 pesos)	Effect of lagged oil price shocks: Coefficient of oilprod88xlop	Effect of lagged oil price shocks: Annual % change dep. var	Pct Tot revenue
	(1)	(2)	(3)
Total Revenue (1)+(2) +(3)	-8,290***	-.013***	100%
	(1,881)	(0.003)	
Municipality revenue (1)	2,136	.015	15%
	(1,478)	(0.010)	
Tax revenue	617	.005	11%
	(1,091)	(0.010)	
Non-tax revenue	1,518***	.045***	5%
	(399)	(0.011)	
Department transfers (2)	35*	.009*	0%
	(20)	(0.005)	
National transfers (3)	-10,462***	-.023***	85%
	(2,214)	(0.004)	
Discretionary Transfers	810***	.018***	8%
	(96)	(0.002)	
Transfers with Mandatory destination (include oil revenue)	-11,273***	-.027***	77%
	(2225)	(0.005)	

Notes: Refer to notes in Table 9.

Table 16. Effect of oil price shocks on municipality expenditures

Dependent variable (Million 2013 pesos)	Effect of contemporaneous oil: Coefficient of oilprod88xlop (t) (1)	Effect of contemporaneous oil: Annual % change dep. (2)	Pct Tot Expenditures (3)
Total expenditures (4)+(5)+(6)	-2,952	-.004	100%
	(7,866)	(0.012)	
Mun gov operations (4)	2,112	.014	22%
	(1,914)	(0.013)	
Debt interest payment (5)	-1,865**	-.070**	2%
	(877)	(0.033)	
Investment (6)	-3,206	-.007	76%
	(5,974)	(0.013)	
Fixed capital accumulation	-1,083	-.005	38%
	(3,104)	(0.015)	
Social investment	-2,123	-.008	38%
	(2,922)	(0.012)	

Dependent variable (Million 2013 pesos)	Effect of lagged oil price shocks: Coefficient of oilprod88xlop (1)	Effect of lagged oil price shocks: Annual % change dep. (2)	Pct Tot Expenditures (3)
Total expenditures (4)+(5)+(6)	6,928***	.011***	100%
	(1,573)	(0.002)	
Mun gov operations (4)	-4,992***	-.034***	22%
	(1,814)	(0.012)	
Debt interest payment (5)	-4,272***	-.161***	2%
	(753)	(0.028)	
Investment (6)	16,177***	.036***	76%
	(3,139)	(0.007)	
Fixed capital accumulation	5,093***	.025***	38%
	(946)	(0.004)	
Social investment	11,084***	.045***	38%
	(3,149)	(0.013)	

Notes: Refer to notes in Table 9.

Chapter II.

Skipping Questions in School Exams: The Role of Non-Cognitive Skills in Educational Outcomes

Abstract

Researchers and education practitioners have become increasingly interested on the relationship between education and students' non-cognitive skills, such as grit, patience and having a growth-mindset. Understanding the development of these skills, however, has been restricted by the availability of objective and inexpensive measures for them. This paper proposes an objective and inexpensive proxy for non-cognitive skills, consisting of the incidence of skipping questions on a statewide standardized achievement test that has no penalties for guessing. The evidence suggests that skipping questions is likely to be related to a reduced level of non-cognitive skills. Conditional on test scores, skipping questions in middle school is related with worse educational outcomes in high school and college, such as increased grade repetition, high school drop-out, delayed graduation and lower attendance rates to 4-year colleges.

1. Introduction

Increasingly, the education and economics literature has been focusing on the role of non-cognitive skills in the school and economic performance of individuals. By non-cognitive skills I mean grit, patience, motivation and a growth-mindset, among other traits different than intelligence. In line with the psychology literature, researchers in economics and education have found that these skills play an important role in individuals' economic performance and in students' education outcomes, in particular.

Almlund, Duckworth, Heckman, and Kautz (2011) present a detailed review of the literature on this topic. The review describes how non-cognitive skills, measured according to the Big Five Personality Traits[1], have been found to be related with more schooling (Goldberg et al. 1998; Eijck and De Graaf, 2004; Lleras, 2008), taking harder classes (Wong and Csikszentmihalyi, 1991), fewer school absences (Lounsbury et al. 2004), graduating from high school (Baron and Cobb-Clark, 2010; Heckman, Stixrud and Urzua, 2006; Cunha, Heckman and Schenach, 2010) and getting higher grades (Poropat, 2009; Duckworth and Seligman, 2005; Duckworth et al. 2010). Furthermore, their review reveals how traits related with conscientiousness, like effortful control and attention, play an important role on predicting test scores (Blair and Razza, 2007; Valiente et al. 2010; Mischel, Shoda and Rodriguez, 1989; Ponitz et al. 2008; Duncan et al. 2007).

To measure adult personality traits and non-cognitive skills, self-report measures are by far the most widely used (Almlund et al. 2011). One example of self-report measures is

[1] The Big Five Personality Traits are a taxonomy of traits that uses factor analysis of observer and self-reports of behaviors. It summarizes personality on five broad components: i) Openness to experience (the tendency to be open to new aesthetic, cultural, or intellectual experiences); ii) Conscientiousness (the tendency to be organized, responsible and hardworking); iii) Extraversion (orientation of interests and energies to the outer world of people and things, positive affect and sociability); iv) Agreeableness (act in a cooperative, unselfish manner); v) Neuroticism (the opposite of emotional stability, which is predictability and consistency in emotional reactions).

the Grit Scale developed by Duckworth, Peterson, Matthews, and Kelly (2007) to measure perseverance and and passion for long-term goals. A second example are the questions, included in the National Longitudinal Survey of Youth (NLSY), that measure respondents' perception of their degree of control over lives (Rotter Locus of Control Scale) and their perception of self-worth (Rosenberg Self-Esteem Scale). Despite the widespread use of self-reported measures, the evidence on their accuracy is mixed. For instance, in the case of motivation, self reports do not change when a behavioral award is removed, even though a response in behavior is indeed observed (Deci, Koestner and Ryan, 1999). On the other hand, self-reported motivation increases when it is expected to, such as when exam stakes are higher (Demars and Wise, 2005).

Alternatives for self-reported measures are parent and teacher reports on a child's observed behavior. For instance, the National Educational Longitudinal Survey (NELS), includes teacher reports on absenteeism, disruptiveness, inattentiveness, and tardiness. Another alternative for measuring non-cognitive skills are task measures, such as the number of seconds a child waits for a more preferred treat in a preschool test of delay of gratification (Mischel et al., 1989). Despite their value, observer reports or task measures can be very expensive for researchers because they may require altering established longitudinal surveys to include new questions or taking measures on the field.

This paper proposes a new proxy for students' non-cognitive skills, one that is low cost and objective, since it can be directly derived from students' test taking behavior. The measure is the incidence of skipping questions on assessment examinations. For this study, I measure the incidence of skipping questions by 7th and 8th grade students on a standardized statewide examination in Michigan. The Michigan Educational Assessment Program (MEAP), which is taken by 3rd to 8th grade students in Michigan public schools, possesses two characteristics that make skipping questions a suitable proxy for non-cognitive skills. First, the exam gives students the same score (zero) whether the student answers the question incorrectly or leaves it unanswered. This means that students would be no worse off by guessing the answer (and getting the question right with 25% probability). Therefore, the incidence of skipping questions does not capture whether some students are strategic responders. Second, time is not a binding constraint in this exam, since schools are instructed to give students as much time as they need to finish the exam[2]. Therefore, skipping is unlikely to capture slow answering, since students have ample time to guess at the last moment, if they want to do so.

Previous research has found a relationship between nonresponse on surveys and non-cognitive skills. For example, item non-response on survey items could reflect lower levels of both cognitive and non-cognitive skills, such as the understanding of the question, the care taken to answer it, the valuation of privacy or the willingness to share information with a stranger (Groves et al. 2009). In support of this argument, Hedengren and Stratmann (2012) find that item non-response in adult surveys is correlated with IQ and self-reported measures of conscientiousness and, furthermore, that this behavior is predictive of earnings and longevity. Similarly, Hitt, Trivitt and Cheng (2016) found that item nonresponse on student surveys are predictive of later educational attainment.

Researchers have also found a relationship between willingness to answer questions on examinations and non-cognitive skills. For instance, Baldiga (2013) suggests that when there are penalties for guessing, the willingness to strategically guess on a high stakes exam could reflect confidence in knowledge of the material or risk aversion. Another example is Segal (2012), who shows that the coding speed test administered without

[2] Informal interviews with middle school educators in the Ann Arbor and Saline area revealed that schools vary in the way they give more time to students. Some schools establish longer exam durations, that go beyond the 1 hour suggested by the state of Michigan. In these cases, students who finish early must stay in their desks reading or doing any other activity in silence. Other schools maintain the standard 1 hour duration, but allow students to move to another room and continue answering the exam. The rest of the students who have already finished proceed with their regular class schedule. The interviews suggested that in these cases only a few students choose to move to another room and continue answering.

incentives to participants in the NLSY relates to intrinsic motivation and that it predicts future earnings. Torija (2012) suggests that correctly answering a very basic numeracy question in an international standardized test (counting dots in a graph) is a measure of willingness to answer. Similarly, Hitt (2015) shows that unpredictable or careless answering on student surveys predicts lower future educational attainment.

Students' decrease in performance over the course of an examination has also been associated with non-cognitive skills. According to Borghans and Schils (2012), when the order of questions is random (so questions do not increase in difficulty) the decline in performance during the test can be used to decompose test scores into cognitive and non-cognitive components. The authors show that this decline overall is related with conscientiousness and agreeableness and that it is correlated with future outcomes such as having a full-time job, academic qualification and smoking and drinking behavior.

This paper contributes to this new literature in several ways. It proposes a proxy for non-cognitive skills that uses standardized achievement tests and rules out behaviors like strategic answering, since the MEAP test has no penalties for guessing. I show that this proxy captures traits other than intelligence, knowledge or speed in test taking. This is because time is not a binding constraint in the MEAP examination and the proxy is predictive of future educational outcomes, even after controlling for students' test scores. In line with Borghans and Schils (2012), this paper proposes an inexpensive proxy for students' non-cognitive skills that could be used in other standardized achievement tests with similar characteristics, in an era of expanded use of education administrative databases. In contrast with their work, it suggests a proxy that can be measured precisely at the student level[3] and can be used even if the order of the questions in the exam is not random.

[3] The large standard errors in the regressions at the student level prevented these authors from estimating a measure of non-cognitive skills by student.

The incidence of skipping questions on a statewide standardized assessment examination could capture non-cognitive skills such as persistence with difficult questions, ability to focus, attention to instructions, intrinsic motivation with learning and/or disengagement with school or examinations. It is not possible to exactly identify which of these traits are captured by non-response. However, patterns observed in the data give some suggestions. Skipping does not seem to be related with age, which suggests that non-response is not associated with non-cognitive skills that develop in a short-time frame (from the 7^{th} to the 8^{th} grade). Neither it is related with question number, suggesting that skipping does not capture the the ability to focus at the end of the exam. On the other hand, skipping does appear to be related with the difficulty of the question, which suggests that skipping is related with persistence when facing challenges, once academic ability is controlled for.

Skipping questions in standardized exams constitutes an objective proxy for a bundle of non-cognitive skills that overcomes the weaknesses of self-report measures. It is also less costly since it does not require collecting observer reports or task measures in the field. This proxy could be used for research in other states or countries that have comprehensive education administrative databases, and that apply standardized tests with no additional penalties for answering a question incorrectly. Such is the case of standardized achievement tests applied in North Carolina and New York City. Moreover, this measure could be useful for detecting the mechanisms through which some educations interventions have effects on student achievement and behavior.

I study the relationship between skipping questions and future educational outcomes using administrative data from the Michigan Department of Education and the National Student Clearinghouse. These data allow researchers to track students in Michigan from kindergarten through college and allows for the observation of all standardized exams students have taken from the 3rd to the 11^{th} grade for each year the student is enrolled in a Michigan public school.

I find that, conditional on test scores (which control for students' academic ability), the incidence of skipping questions in the 7th and 8th grade is consistently related to educational outcomes in high school and college. A student who skips multiple questions in one or more exams[4] in the 7th and 8th grades is about six percentage points less likely to graduate on-time from high school, two percentage points more likely to drop out of school, three percentage points more likely to repeat grades in high school and two percentage points less likely to enroll in a any type of college, relative to a comparable student who did not skip any questions in 7^{th} or 8^{th} grade. These results are robust to the definition of skipping incidence or to the measurement of cognitive ability as captured by test scores. This evidence suggests that the incidence of skipping questions in exams serves as a proxy for traits that are not related with intelligence or knowledge, which are likely to be non-cognitive skills[5].

2. Methodology

The objective of this paper is to study the relationship between the incidence of skipping questions in exams and future educational outcomes. I accomplish this by estimating OLS models with several measures of educational attainment in the left hand side and the incidence of skipping questions in the right hand side, together with other covariates.

Specifically, I estimate the equation

$$Y_{ij} = \beta_0 + \beta_1 skip_{ij} + \beta_2 f(S_{ij}) + \beta_3 X_{ij} + \varepsilon_{ij}$$

[4] Students enrolled in grades 7 and 8 during this period are expected to take 5 assessment exams, described in further detail later.

[5] Even though it would be interesting to use this proxy to target children for supports, I found that the proxy does not explain a significant fraction of the variation in educational outcomes, beyond the standard socio-demographic characteristics.

Where Y_{ij} denotes the educational outcome of student i in school j, $skip_{ij}$ is a vector of measures for the student's incidence of skipping in grades 7th and 8th, $f(S_{ij})$ is a quadratic function of average test scores in all exams taken in 7th and 8th grades, and X_{ij} is a vector of covariates, which includes student demographic and school characteristics.

The educational outcomes consist of several measures observed in high school and college. In high school I estimate the probability of grade repetition, dropping out, and on-time high school graduation[6] as well as predictive performance on high school standardized exams (MME and ACT). For college outcomes, I consider enrollment in college one year after expected on-time high-school graduation[7] in any postsecondary institution, as well as specific 4-year or 2-year college enrollment.

The main variable of analysis, $skip_{ij}$, is a vector of indicators that assigns a student to one of three mutually exclusive groups according to the severity of his skipping behavior. The first indicator is skipping only one question on exactly one exam, which is the least severe group. The second is skipping multiple questions on exactly one exam, and the third, and most severe measure, is skipping questions in multiple exams. This the most severe measure because it represents persistence on skipping behavior. This measure is also less likely to reflect a student who had a single "bad day". The excluded category is thus the group of students who never skip a single question on any exam.

One critical component of the equation above is the inclusion of the function of average test scores, which is incorporated to control for academic ability. This is a key aspect of the analysis because the students who skip questions could be precisely those with lower

6 For the analysis sample (2006 7th grade cohort), a student with a normal grade progression would have started the 9th grade in the 2007-2008 academic year and graduated on-time by the 2010-2011 academic year. "On time" is reflective of the academic year in which a student begins the 9th grade, so a student could still graduate on time even if he repeated the 7th or 8th grade.

[7] For a student with a normal grade progression in the cohort analyzed, this means enrolling in college any time in the 2011-2012 academic year.

levels of intelligence or academic knowledge, since a student who knows how to answer a question would be unlikely to skip that question.

In order to isolate this confounding factor, I control for the average test scores that students received in all exams taken in the 7th and 8th grades. Although test scores are not a perfect measure of cognitive skills, they have been largely used in the literature to measure academic ability. By controlling for test scores, the coefficient on the incidence of skipping would reflect the importance of traits different than intelligence and academic knowledge, as for example, non-cognitive skills.

Controlling for test scores is equivalent to finding a comparable student whose academic ability is equivalent to the skipper's. However, controlling for actual test scores might understate the academic ability of the student who skips. This is because he receives zero points for the questions he skips, which assumes that he would have gotten the question wrong had he answered it. Therefore, it is necessary to adjust the test scores to account for this.

I adjust the standardized scores by simulating additional scenarios, had the student answered the question. The student could have at least guessed randomly and gotten the right answer with 25% probability.[8] Alternatively, he could have made an educated guess by eliminating incorrect answers, which is probably related to the rate at which he got the rest of the exam correct (e.g. a student who got 75% of the rest of the test correct may make better educated guesses than a student who got 25% of the rest of the exam correct). I modify test scores by adjusting skippers' proportion of correct answers and then translating these adjusted percentages into alternative standardized test scores.

[8] This number comes from the chance of getting the correct answer in a multiple-choice question with 4 options.

The adjustment of the percentage of correct answers is performed in three different ways: (i) increasing the number of correct answers assuming that the student randomly guessed the answer (e.g., increasing the number of correct answers by 0.25 for each skipped question); (ii) increasing the number of correct answers by the rate at which the student got correct the questions he attempted (e.g. if he got right 60% of the questions he did not skip, I increase the number of correct answers by 0.6 for each skipped question); and (iii) assuming every skipped question would have been answered correctly (e.g. increasing the number of correct answers by 1 for each skipped question). I believe that student's true academic ability lies somewhere between scenarios (i) and (ii). However, I provide results for scenario (iii) to present an upper bound for the estimates.

Each of the three adjustments to the percentage of correct answers is then used to create an alternative standardized scaled score. The alternative scores are created using non-skippers' information, since the specific function that maps percentage of correct questions into scaled test scores is unknown. I estimate the alternative scores by first regressing standardized scaled scores on the percentage of correct answers for the students who do not skip questions. Then, I use the estimated coefficients to make two out-of-sample predictions for skippers' standardized scaled scores, one using the adjusted percentage of correct answers and one using the unadjusted. The difference between the two predicted scores is then added to the actual standardized scaled score and, as a result, there are three final adjusted standardized scale scores, one for each way of adjusting the percentage of correct answers (options (i) to (iii) above).

Finally, I include other covariates in the regression, such as students' race, gender, poverty status (as measured by ever been eligible for free/reduced price lunch), age and participation in special education and limited English proficiency programs. I also include fixed effects for the school where the student was in the 7th and 9th grade. This set of covariates is added one by one, in order to identify how sensitive the coefficient of interest (the incidence of skipping) is to the inclusion of these variables.

3. Data

The data for this study come from the Michigan Department of Education (MDE) and the Center for Educational Performance and Information (CEPI) at the State of Michigan. Several sources of cross-sectional MDE and CEPI administrative databases were joined together to form a longitudinal student database for the 2006 cohort of first-time 7th grade students. This longitudinal database tracks students forward with respect to their school enrollment, achievement scores, high school graduation and college attendance.

The CEPI administrative data includes the Michigan Student Data System (MSDS), which provides information on student demographics such as race, gender, free/reduced price lunch eligibility (a measure of poverty status) and students' final exit status. The exit status allows us to identify the standing of the student the last time he was seen in the administrative data system. Specifically, the exit status reports if the student graduated from high school, moved out of state or to a private school, dropped out to obtain a GED, among others. The MSDS also identifies if the student was in a special education or limited English proficiency program.

The MDE data includes two sources of assessment information. The first one is the Michigan Educational Assessment Program (MEAP), which provides test scores data for grades 3-8 between 2004-2012. The MEAP test is administered in various grade-subject combinations. Not all subjects are tested in each grade and, within a grade, testing of some subjects started later in that period. For instance, math and reading are assessed every year, while science is only assessed in grades 5 and 8. Also, math was only initiated for testing in the 7^{th} grade in the 2006 academic year. The second assessment database is the Michigan Merit Examination (MME) database, which provides assessment data for 11th grade students in subjects such as mathematics, English, reading and science, among others. The MME also includes a full ACT component, which is the college entrance exam applied in the state of Michigan, equivalent to the

SAT.[9] Since ACT testing became mandatory (and free) in Michigan public high schools in 2007, all students in the analysis sample were expected to take the ACT when reaching the 11th grade.[10]

The MDE and CEPI administrative data was supplemented with postsecondary enrollment and retention information obtained from the National Student Clearinghouse (NSC). The NSC tracks students' college enrollment, including the type of institution he was enrolled in (public, private, 4-year or 2-year). The NSC data also allows measuring persistence, since it identifies how long students were enrolled in college.

Measuring skipping and education outcomes

The proxy for non-cognitive skills - the incidence of skipping behavior on assessment exams – comes from the MEAP examination database. The MEAP test includes two variables with the necessary information to determine if a question was skipped. The first variable is the string of question answers, which records the responses that students marked (A to D) or a blank if no answer was provided. The second variable is the string of scored question answers, which consists of 0s and 1s, 1s for all questions correctly answered and 0s for all questions incorrectly answered or left blank. A skipped question, therefore, is an item that has a blank on the string of answers and a zero on the string of scored answers[11].

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[9] Questions left blank in the ACT do not receive the same score than incorrect questions. For this reason, the ACT exam is not used to construct the skipping measure. Instead, the ACT scores are used to assess the relationship between skipping questions in middle school exams and high school achievement.

10 More detailed information about the structure and background of the data sources can be found in Dynarski, Frank, Jacob and Schneider (2013).

[11] Both the string of answers and the scored string of answers are needed to identify skipped questions, because of the existence of field items. Field items are new questions included in the exam that are being tested and therefore do not count towards the final score. Field items are registered in the string of answers as blanks. I can distinguish field items from skipped questions, because field items have a blank on the string of scored answers, while skipped questions have a zero.

I am interested in measuring the relation between the incidence of skipping in assessment examinations during middle school and future educational outcomes. The outcomes of interest are on-time high school graduation (4 years after starting 9th grade), delayed high school graduation (5 years after), grade repetition, performance in high school standardized examinations, together with college enrollment. In order to measure the relation between skipping behavior and future high school and college outcomes, it is necessary to identify skipping behaviors before students enter to high school (in middle school, for example) and following them into high school and college. To accomplish this, I measure the incidence of skipping questions in middle school, when students were in the 7th and 8th grades.

Students in the 7th and 8th grades could have skipped questions in up to 5 exams, since they take math and reading examinations in both grades and science in the 8th grade only. On average, the math test consist of 60 questions and the reading test consists of 30 questions.[12]

Sample

The analysis sample for this study consists of the cohorts of students who were first-time 7th graders in Michigan public schools in 2006 through 2009[13]. I restricted the sample to students who were enrolled in Michigan public schools through at least the ninth grade, so I could analyze their education outcomes in high school. These cohorts were chosen first to maximize middle-school test taking, since the 7th grade math test was first offered in 2006, and second, to follow students through college, given that the 2008-2009 7th grade cohort could be in first year of college by 2015.

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[12] The number of questions in a test varies by grade and year. For instance, the math test has between 52 and 66 questions in the 7th grade and between 41 and 60 questions on the 8th grade.

[13] The year corresponds to the spring semester of the academic year. That is, 2006 denotes the 2005-2006 academic year.

I exclude students who moved out of the state, or transferred to private schools or home schooling, since I cannot track them once they exit the system. I also exclude students in non-regular schools or in schools with less than 10 students per cohort. Finally, I exclude students with missing scores in any of the five exams taken during middle school. These students are excluded because they have less opportunities to skip questions in exams and, therefore, their skipping rate might be underestimated.[14] This leaves us with 381,421 unique students included in the estimation sample. Henceforth, I will call the analysis sample the 2006-2009 7^{th} grade cohorts, where the year corresponds to the calendar year when the spring semester took place.

4. Summary Statistics

Skipping questions on standardized exams is a behavior characteristic of a small percentage of the population of students. As Table 17 shows, 16% of the students skip at least one question in one exam (Columns 3 through 5). Of these, a third correspond to students skipping at higher frequencies, equally divided between those who skip multiple questions in a single exam (Column 4) and those who skip in multiple exams (Column 5). The remainder skip only one question on a single exam during the 7th and 8th grades (Column 3).

What are the characteristics of the students who skip? Skipping even one question in one examination (out of five) provides meaningful separation from students who never skip. As Panel A in Table 18 shows, the group of students skipping exactly one question is more likely to be black than the group who never skips (24% vs. 14%), be eligible for free or reduced lunch (60% vs. 47%) and belong to a special education program (15% vs. 10%). Further, this group performs worse in middle school standardized achievement

[14] Table A.1. in the appendix compares the characteristics of students who miss one or more middle school exams (out-sample) with students who do not miss exams (in-sample). The Table reveals that those students who miss exams belong to disadvantaged groups at higher rates and perform almost one standard deviation below the mean in standardized achievement tests. Missing school exams itself could also reflect low levels of non-cognitive skills, but I turn aside it to focus only on the skipping behavior.

tests. For instance, those who skip one question perform -0.3 standard deviations (sd) below the mean, compared to those who never skip, who perform +0.09 above the mean.15

Consistent with the patterns observed in middle school examinations, skippers' also have worse educational outcomes in high school and college. According to Panel B, those who skip one question perform -0.33 sd below the mean on the ACT, compared to +0.06 sd above the mean for never skippers. They also repeat high school grades at higher rates (16% vs. 10%), take longer to graduate from high school (9% still enrolled in HS 5 years after starting vs. 6%) and enroll in college at lower rates (54% vs. 65%). These differences between skippers and non-skippers are stronger when the incidence of skipping questions increases (Columns 4 and 5).

Since the characteristics that separate skippers from non-skippers are likely to be correlated, I perform a regression analysis to predict skipping, including all the mentioned characteristics as covariates. As Table 19 shows, race/ethnicity and student academic performance have the highest explanatory power. An African-American student is between one and four percentage points more likely to skip questions, depending on the measure of skipping considered. A student who scores one standard deviation above the mean on middle school examinations is between 2 and 4 percentage points less likely to skip questions, all other things equal. Including all the characteristics as covariates decreases the correlation of skipping with poverty and special education status. For instance, controlling for race and academic achievement,

[15] These numbers are calculated based on the average standardized score in all exams taken in middle school.

free/reduced lunch and special education students are only about one percentage points more likely to skip questions.[16]

Surprisingly, I did not find strong evidence of males skipping at higher rates than females. Females are only one percentage point less likely to skip one question (Column 1), and 0.04 percentage points less likely to skip two or more questions (Columns 2 and 3). Since the literature suggests that the higher educational achievement of females is partially explained by differences in non-cognitive skills (Jacob, 2002; Cornwell, Mustard and Van Parys, 2013), this suggests that the non-cognitive skills associated with skipping questions in standardized exams might not be gender specific.

What could skipping measure?

Observed patterns in the data suggest that skipping does not capture traits specific to a particular age, since students in the 7th grade skip questions at the same rate than students in the 8th grade (Figure 9).

Skipping is related to the subject evaluated. As Figure 9 reveals, students are almost two times more likely to skip questions in math exams (5%) than in reading exams (3%). The majority of them skip one question, but a non-negligible portion skips two or more questions. For example, out of the 5% of students who skip questions in the math exam on 7th grade, a quarter skip two or more questions.

In addition, skipping is related to the difficulty of the question, as pupils tend to skip questions that other pupils get incorrect.[17] As Figure 10 and 11 show, there is a positive relationship between the percentage of 7th grade students that answer incorrectly a math or reading question and the percentage who skip it. This positive relationship between

[16] This regression also suggests that skippers are more likely to be Asians and Hispanics. However, Table 1 reveals that the representation of these races/ethnicities is very small in the sample, so I prefer to disregard these significant differences in skipping.
[17] The percentage of students who answer incorrectly does not include those who skip

difficulty and skipping reinforces the importance of controlling for overall test taking performance. Once test scores are controlled for, this relationship could suggest that skipping behavior captures lack of perseverance when the student faces challenges.

Finally, there is mixed evidence on whether skipping captures tiredness at the end of the test. Figure 12 and 13 illustrate how the percentage of students who skip a question relates to the question number (or question order). While pupils skip more questions at the end of the reading exam, the same does not happen in the math exam, which is the subject with higher skipping incidence.

5. Results

The objective of this paper is to study the relationship between the incidence of skipping questions on standardized statewide examinations and future educational outcomes. As it was stated in section 3, I accomplish this by running OLS regressions with future educational outcomes on the left hand side and skipping incidence on the right hand side, together with other covariates.

Table 20 presents the results of the regressions for on-time high school graduation (e.g. graduating after 4 years of first entering the 9^{th} grade). The model is built up by slowly adding the different sets of covariates. It starts with column 1, which includes only demographic variables as explanatory variables. This column confirms a well-established fact in the literature, that black and male students are less likely to graduate on-time from high school. However, when academic ability is added in column 2 (with a quadratic on random-guessing adjusted test scores), the coefficients in column 1 change dramatically. Conditional on cognitive skills, black students are more likely to graduate on-time from high school, confirming the findings in Cameron and Heckman (2001). The same is true for special education students.

Column 3 adds the main variable of interest, which is the incidence of skipping in exams. This variable is included by adding a set of indicators which define mutually exclusive groups of skippers: those who skipped exactly one question in one exam, those who skipped multiple questions in exactly one exam, and those who skipped at least one question on multiple exams, which I consider to be the most severe measure of skipping. The omitted category consists on the group of students who do not skip a single question on any of the baseline examinations[18].

The coefficients on the skipping indicators are negative, statistically significant and have meaningful magnitudes. For instance, a student who skips only one question in one exam is 2.4 percentage points less likely to graduate on-time from high school than an observationally similar student who does not skip any question. The relationship gets stronger as the severity of skipping increases; a student who skips several questions in one exam is 4.6 less likely to graduate on-time than a non-skipper, while a student who skips at least one question on multiple exams is 6.9 percentage points less likely to do so.

The inclusion of the skipping indicators has negligible effects on the coefficients of other covariates, which suggests that skipping captures variation that was formerly embedded in the error term. This, however, does not imply that skipping adds explanatory power to the prediction of on-time high school graduation, since the R-squared does not change significantly when adding this variable.

Finally, columns 4 and 5 add cohort and 7th grade-school fixed effects. The results are largely unchanged by the inclusion of these fixed effects, which suggests that the relationship between skipping and graduation does not arise from specific school practices or resources, but it rather comes from differences between individual students.

[18] The incidence of skipping was also added using continuous measures of non-response, such us the percentage of questions skipped and the percentage of exams/years when the student skipped. The results, not presented here, are qualitatively the same and are available in the appendix/upon request.

In Table 20 I control for cognitive skills using the random-guessing adjusted test scores. This adjustment assumes that if the student had answered the questions he skipped, he would have gotten them right at random. As mentioned in section 3, other alternatives to control for academic ability include using actual test scores, using percent-correct adjusted test scores (e.g. assuming she would have gotten skipped questions right at the same rate she got right the rest of the exam) and all-correct adjusted scores (e.g. assuming she would answered skipped questions right). For comparison purposes, Table 21 presents the results with these alternative adjustments. The table displays the estimated coefficients on the skipped indicators for each adjustment, using the preferred specification, which is column (5) in Table 20.

As I move through the different adjustments (from column 1 – actual test scores, to column 4 – all-correct adjusted test scores), the estimated coefficients get higher in magnitude and more significant. For example, a student who skipped multiple questions in exactly one exam is 4.6 percentage points less likely to graduate from high school on-time, when actual test scores are controlled for. The coefficient increases to 5.2 percentage points if I control for random-guessing adjusted scores, to 5.8 percentage points if I control for the percent-correct adjusted scores and to 7 percentage points if I control for all-correct adjusted scores (column 4). This increase in the magnitude of he coefficient is natural, because the adjustments correct any under-estimation of skippers academic ability, which implies comparing them with students of higher academic ability. Among these options, the preferred is random-guessing adjusted test scores, because it constitutes a middle point between assuming that the skipper would have gotten all the skipped questions wrong or all of them right.

Having presented the results for on-time high school graduation with different test scores adjustments, Table 22 proceeds to summarize the results for the rest of the outcomes. The outcomes, displayed in the columns, include grade repetition in high school, continued enrollment after 4-years of starting the 9^{th} grade, college enrollment and performance in high school standardized examinations. In this table I control for academic ability using random-guessing adjusted test scores. The results do not change using optional adjustments for test scores.

Table 22 shows that a student who skips questions in exams is more likely to repeat grades in high school, at rates that range from 1.6 and 3.6 percentage points, depending on the incidence of skipping considered. Consistently, this student is more likely to still be enrolled in high school after 4 years of starting the 9^{th} grade. Furthermore, this student is less likely to enroll in any type of college, at rates that range from 1.4 and 2.7 percentage points.

These results are all qualitatively the same and the magnitudes and significance of the coefficients reinforce the relationship between skipping and future educational outcomes. One exception, however, is the performance on high school examinations, since the evidence for these outcomes depends on the subject considered. Skippers perform worse in math exams regardless of the skipping incidence, from 0.03 to 0.05 standard deviations below non-skippers, which is consistent with the previous results. In contrast, students who skip multiple questions in exactly one exam (row 2) have *higher* reading test scores. These results are intriguing and encourage digging in further in the role of skipping questions in reading examinations in particular.

6. Conclusions

The incidence of skipping questions on standardized examinations in Michigan is related to reduced rates of on-time high school graduation, increased high school drop-out and repetition rates, and reduced rates of college enrollment. This relationship holds under a wide range of model specifications, different controls for baseline academic achievement, and definitions of the variable of interest. Further, more severe definitions of skipping incidence are related to more severe educational outcomes.

In this Michigan examination, there are no penalties for guessing, the test is multiple choice, and students are not bound by a time constraint. Thus, students are always weakly better off guessing than leaving questions blank and should not be constrained by time or ability from doing so. Further, even if there was an effective time constraint (students wishing not to take the "extra time" they are given), it would take them only minimal effort to fill in the blanks randomly at the end of the exam. This structure of the exam, combined with our controls for baseline test scores, lead us to believe skipping incidence is capturing something different than intelligence and academic ability, unobserved non-cognitive skills which are important for academic success.

Surprisingly, I did not find strong evidence of males skipping at higher rates than females. Females are only 1 percent point less likely to skip only 1 question (Columns 1), and 0.04 percent points less likely to skip 2 or more questions, compared to males (Columns 2 and 3). Although the literature suggests that the higher educational achievement of females is partially explained by differences in non-cognitive skills (Jacob, 2002; Cornwell, Mustard and Van Parys, 2013), I find that females are only 0.8% less likely to skip questions. This suggests that the non-cognitive skills associated with skipping questions in standardized exams are not gender specific.

This paper has proposed a low-cost objective measure of non-cognitive skills that may allow us to get at mechanisms through which other programs work to increase overall test scores, which makes a good starting point for additional research to identify which non-cognitive skills are important to academic success.

This study does have the limitation of increasing only slightly the R-squared statistic, when the skipping incidence is included as a predictor of educational outcomes. As a result, skipping does not help to better identify students who are at risk of dropping out from high school, which would be useful for schools and policy-makers. More research is needed to identify if there exists a population of students for which skipping incidence does add predictive value.

In addition, it is difficult to untangle the bundle of non-cognitive skills skipping incidence may represent: for example, perseverance, motivation, attention to instructions, competitiveness, or composure. Fortunately, the availability of rich administrative databases allows for a deeper investigation into student test taking than just a simple observation of the final score. Patterns of answers and non-response may offer opportunities for researchers to develop a richer understanding of the relationship between test taking and future academic achievement.

The measure here proposed should not be used for accountability purposes, or to target education policies or evaluate programs that intent to boost non-cognitive skills. This is because it could incentivize practitioners to make sure students do not leave questions blank in standardized exams. This would cause an immediate decrease in skipping incidence, without an underlying improvement on students non-cognitive skills. Rather, this measure serves as an example of serious behaviors that can be predicted from minor behaviors, opening an entire avenue for research in the topic of non-cognitive skills.

One area of future research is to identify which non-cognitive skills are dominant or lacking through patterns of skipping behavior. For example, skipping difficult questions might reflect a lack of perseverance or composure. On the other hand, skipping a string of questions at the end of the exam may reflect a lack of competitiveness or poor attention to directions as these students easily could have filled in answers randomly in the final minute of the examination. Once identified, researchers could test whether some of these non-cognitive skills are more or less related to future academic success or whether some behaviors are more or less persistent across examinations. Further, applying measures of non-cognitive skills on the field using standard psychological metrics, would allow to study how skipping incidence relates to this measures and, therefore, which non-cognitive skills is it likely to apply. Finally, explicit models of test taking behavior may also lead to predictions that can be tested with administrative data.

7. References

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8. Figures and Tables

Figure 9. Percentage of students skipping at least one question, by subject and grade

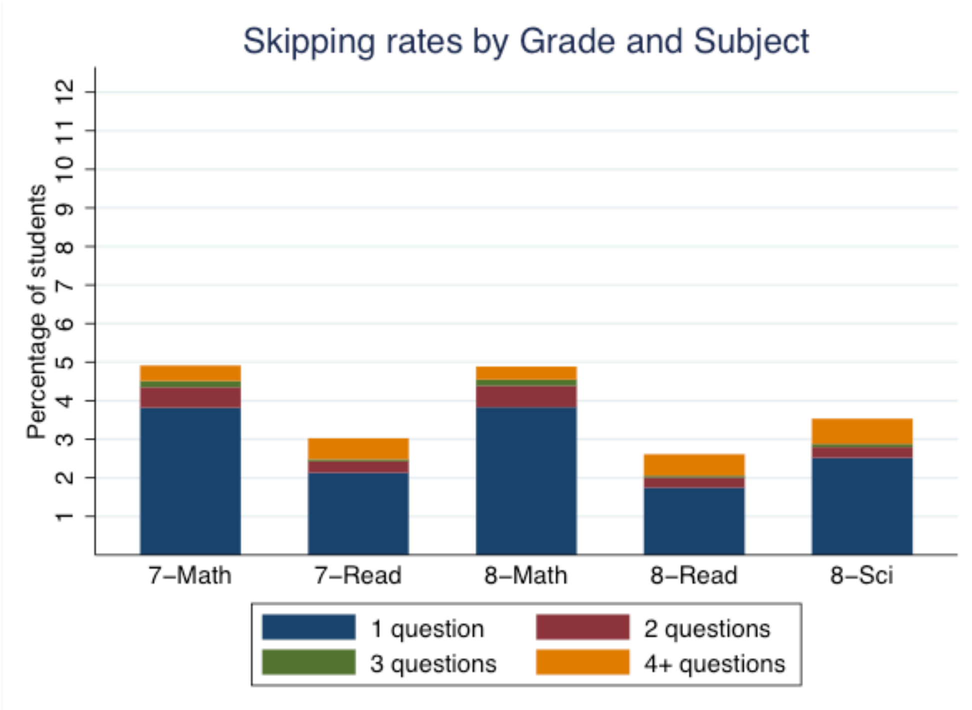

Figure 10. Skipping rates in Math and difficulty of the question

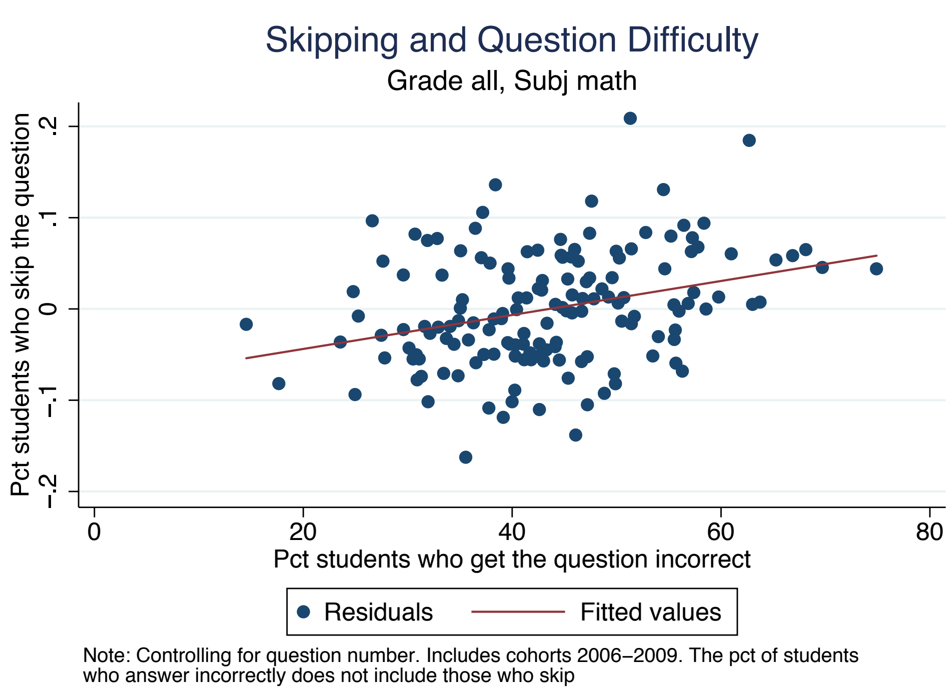

Figure 11. Skipping rates in Reading and difficulty of the question

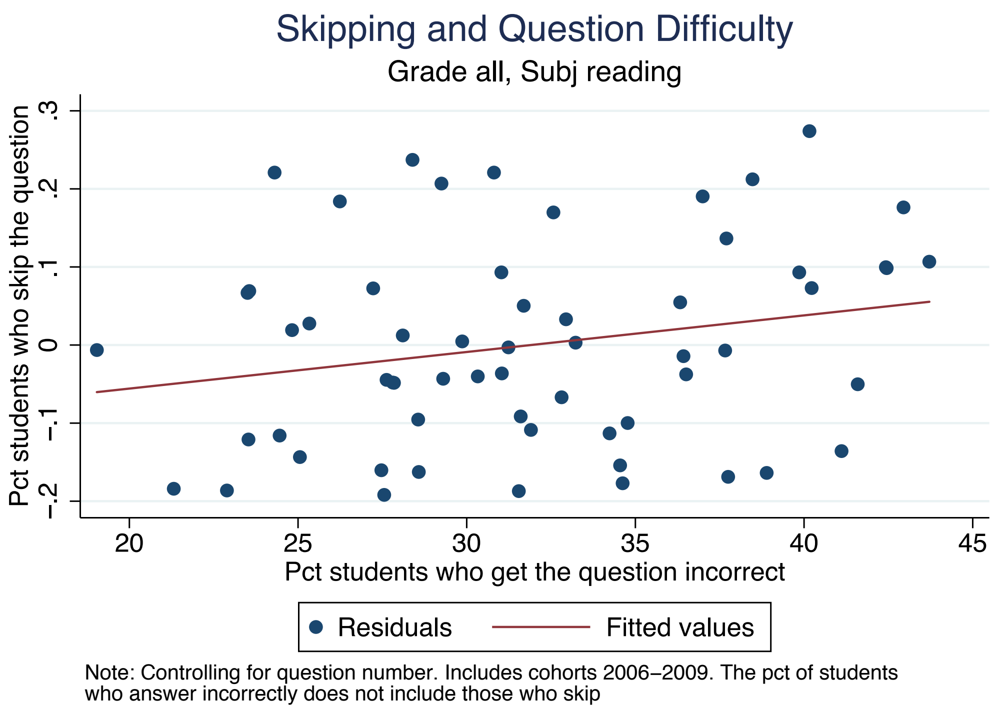

Figure 12. Skipping rates and question order in Math

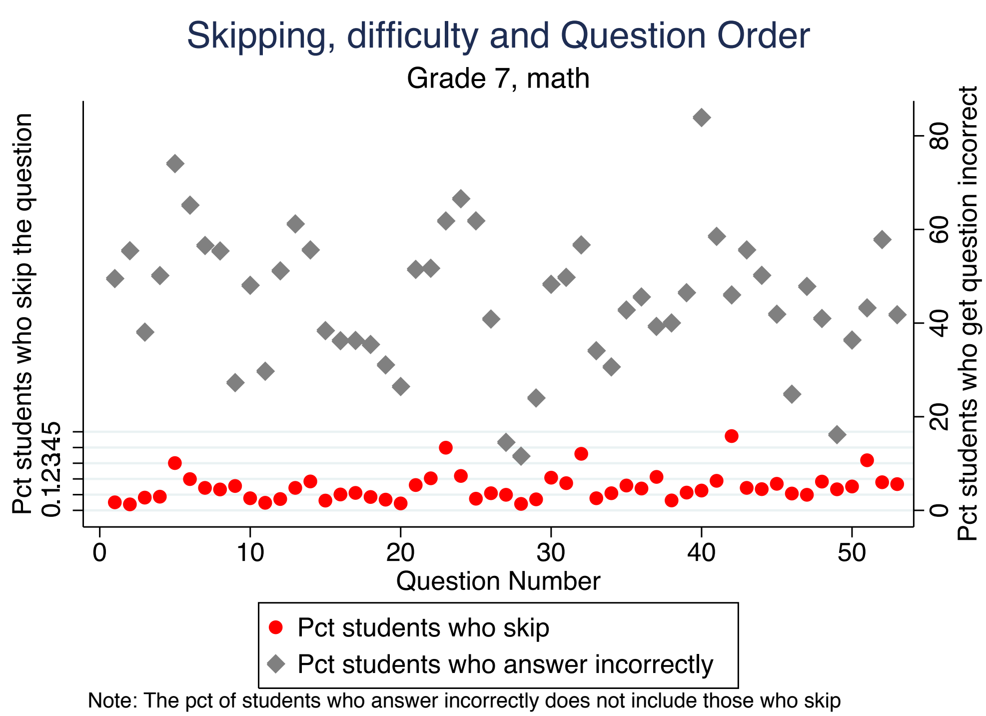

Figure 13. Skipping rates and question order in Reading

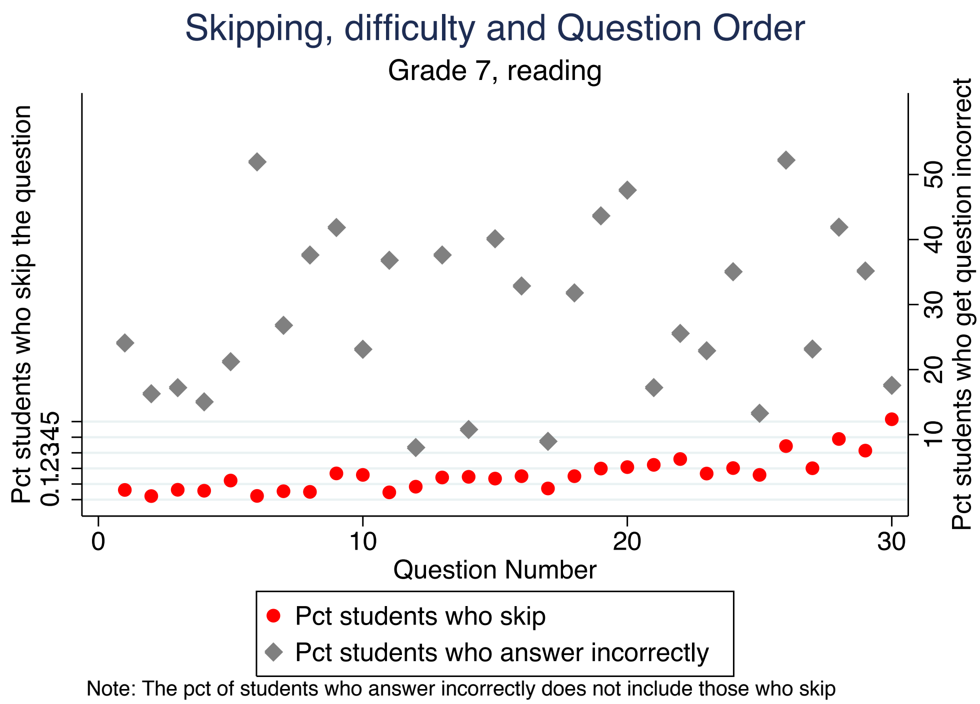

Table 17. Skipping frequency: Number of exams where students skip

N. Students who skip at least one question in:	**Freq.**	**Percent**
0 exams	328,847	84%
1 exam	53,412	14%
2 exams	8,393	2%
3 exams	1349	0%
4 exams	234	0%
5 exams	40	0%
Total	392,275	100%

Table 18. Characteristics of students who skip questions, by different skipping incidences

Panel A. Covariates

Variable	All	Never Skip Questions	Skip Exactly 1 Question Ever	Skip 2+ Questions in 1 exam only	Skip Questions in 2+ exams
	(1)	(2)	(3)	(4)	(5)
Number Students	384,729	323,165	39,791	11,898	9,664
Pct students	100%	84%	10%	3%	3%
Number Schools (1)	952	952	947	916	920
White	76.5%	78.8%	67.6%	64.6%	51.1%
Black	16.0%	13.8%	24.2%	28.3%	42.1%
Hispanic	4.1%	3.9%	5.2%	4.5%	4.6%
Asian	2.3%	2.4%	1.9%	1.5%	1.3%
Ever Free/Reduced Lunch Status	50.0%	47.7%	59.6%	62.7%	72.0%
Limited English Proficiency	2.8%	2.6%	3.6%	3.3%	3.7%
Special Education	10.8%	9.6%	15.5%	17.4%	22.5%
Female	50.2%	50.9%	47.2%	46.7%	43.7%
Age	13.30	13.29	13.32	13.33	13.37
Middle School Std Test Scores					
Av Math MS (2)	0.02	0.10	-0.33	-0.43	-0.75
Av Reading MS	0.02	0.10	-0.31	-0.45	-0.73
Av 7th Grade (3)	0.02	0.10	-0.32	-0.44	-0.76
Av 8th Grade	0.02	0.10	-0.32	-0.44	-0.74
Overall Av (4)	0.02	0.10	-0.32	-0.44	-0.75

Table 18. Characteristics of students who skip questions, by different skipping incidences (cont)

Panel B. Outcomes

Variable	All	Never Skip Questions	Skip Exactly 1 Question Ever	Skip 2+ Questions in 1 exam only	Skip Questions in 2+ exams
	(1)	(2)	(3)	(4)	(5)
HS Std Test Scores (5)					
Math MME	0.01	0.08	-0.30	-0.37	-0.74
Reading MME	0.01	0.07	-0.27	-0.30	-0.62
Science MME	0.01	0.08	-0.30	-0.37	-0.70
Composite ACT	0.01	0.08	-0.32	-0.36	-0.68
Math ACT	0.01	0.08	-0.31	-0.36	-0.63
Reading ACT	0.01	0.07	-0.27	-0.29	-0.56
Science ACT	0.01	0.08	-0.30	-0.35	-0.66
HS Grade Repeat	11.2%	9.8%	16.1%	19.5%	24.5%
HS Graduation					
Graduated after 4yr (6)	86.0%	87.6%	79.9%	76.3%	69.1%
Still enrolled after 4yr (7)	10.1%	9.0%	14.5%	16.7%	21.8%
Graduated after 5yr (8)	88.2%	89.7%	83.1%	79.8%	73.1%
Still enrolled after 5yr	6.5%	5.7%	9.5%	10.9%	15.2%
College Enrollment (9)					
4yr College	38.3%	40.5%	29.0%	26.0%	19.1%
2yr College	23.9%	23.7%	24.8%	24.9%	23.4%
Any College	62.2%	64.3%	53.8%	50.9%	42.5%

Notes: The sample consists of all students who started the 7th grade for the first time between 2006 and 2009. All test scores are standardized by grade and year. (1) Number of schools with at least one student in the corresponding subgroup. (2) Av. Math standardized test scores across Middle School (MS) grades (7-8). (3) Av. 7th grade standardized test scores across subjects (Math and Science). (4) Av. standardized test scores across MS grades and subjects. (5) The high school achievement test refers to the MME examination designed by the state of Michigan. The ACT test is embbeded within the MME (6) Defined as having graduated from high school (HS) 4 years after starting 9th grade for the 1st time. (7) Still enrolled in HS 4 years after starting 9th grade for the 1st time (8) Having graduated from HS 5 years after starting 9th grade. (9) Enrolled in a 4yr college 1 year after on-time HS graduation.

Table 19. Who skips? Predictors of skipping only one question in one single exam

	Skip Only 1 Question Ever (1)	Skip 2+ Questions Ever (2)
Black	0.031***	0.007***
	(0.002)	(0.001)
Hispanic	0.018***	-0.002
	(0.003)	(0.002)
Asian	0.014***	0.001
	(0.003)	(0.002)
Ever Free/Reduced Lunch	0.005***	0.002***
	(0.001)	(0.001)
Female	-0.011***	-0.004***
	(0.001)	(0.001)
Special Education	0.010***	0.005***
	(0.002)	(0.001)
Limited English Proficiency	0.001	-0.006***
	(0.004)	(0.002)
Age	0.001	0.001*
	(0.001)	(0.001)
Av. Std. Middle Sch Test Scores	-0.040***	-0.016***
	(0.001)	(0.000)
Observations	384,729	384,729
R-squared	0.024	0.017

Notes: The sample consists of all students who started the 7th grade for the first time between 2006 and 2009. Fixed effects at the 7th grade school level are included. All test scores are standardized by grade and year. Av. Std. Middle School Test Scores represents average test scores across middle school grades (7-8) and subjects (Math, Reading and Science).

Table 20. The relationship between skipping incidence and on-time high school graduation

Dependent variable: On-time high school graduation	**(1)**	**(2)**	**(3)**	**(4)**	**(5)**
Skip Exactly 1 Question Ever			-0.022***	-0.021***	-0.021***
			(0.002)	(0.002)	(0.002)
Skip Exactly 2+ Questions			-0.052***	-0.052***	-0.049***
			(0.003)	(0.003)	(0.003)
Black	-0.039***	0.009**	0.012***	0.013***	0.038***
	(0.005)	(0.005)	(0.004)	(0.004)	(0.004)
Ever Free/Reduced Lunch Status	-0.151***	-0.113***	-0.113***	-0.113***	-0.107***
	(0.003)	(0.002)	(0.002)	(0.002)	(0.002)
Female	0.047***	0.049***	0.049***	0.049***	0.049***
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
Av. Std Test Scores		0.091***	0.088***	0.088***	0.085***
		(0.002)	(0.002)	(0.002)	(0.002)
Observations	384,729	384,729	384,729	384,729	384,729
R-squared	0.090	0.126	0.127	0.128	0.147
Cohort FE				x	x
7th grade school FE					x

Notes: All specifications include indicators for limited english proficiency, special education, and a quadratic in age. Errors clustered at the 7th grade school level.

Table 21 Skipping and 4-Year High School Graduation Under Alternative Adjustments to Baseline Test Scores

	No Score Adjustment	Random Guess Score Adjustment	Percent Correct Score Adjustment	All Correct Score Adjustment
	(1)	(2)	(3)	(4)
Skip Exactly 1 Question Ever	-0.021***	-0.022***	-0.023***	-0.025***
Skip Multiple Questions on Exactly 1 Exam	-0.046***	-0.052***	-0.058***	-0.070***
Skip at least 1 question on Multiple Exams	-0.058***	-0.065***	-0.070***	-0.083***

Notes: Coefficient Estimates correspond to the same specification as in column 5 in Table 20 above

Table 22 The relationship between skipping and several educational outcomes

	Repeat Grade high school	On-time HS graduation	Enrolled HS after 4 years	Enrolled college	Std. HS Math Test Scores	Std. HS Reading Test Scores	Std. HS Science Test Scores
Skip Exactly 1 Question	0.016***	-0.021***	0.015***	-0.014***	-0.034***	0.010**	-0.036***
	(0.002)	(0.002)	(0.002)	(0.002)	(0.004)	(0.004)	(0.004)
Skip 2+ Questions	0.036***	-0.049***	0.030***	-0.027***	-0.048***	0.055***	-0.025***
	(0.003)	(0.003)	(0.003)	(0.004)	(0.007)	(0.006)	(0.007)
Observations	384,729	384,729	384,729	384,729	357,965	359,379	358,660
R-squared	0.137	0.147	0.102	0.207	0.548	0.578	0.555
Sample mean	0.112	0.860	0.101	0.622	0.0135	0.0126	0.0131

Notes: Specifications match Model (5) in Table 20 above.

Chapter III.

Childcare Programs and Their Spillover Effects on Older Siblings

Abstract

The study of childcare programs has focused on their effect on the wellbeing of young children and their mothers. However, less is known about how these programs affect other household members. This research paper investigates the effect of a public childcare program in Colombia on older sisters' time use and educational attainment. Following Attanasio et al. (2006), who study the effect of this childcare program in young children, I use the time from the household to the closest childcare center as an instrument for program participation. The results reveal that having young siblings participating in the public childcare program is related to an increase in the time that older sisters spend on housework. A possible mechanism underlying this phenomenon is female labor supply. Specifically, childcare could incentivize mothers' participation in the labor force, which prompts them to transfer their housework responsibilities to their older daughters. This change in girls' time use, however, does not affect their educational attainment, since no significant effects are found on grade repetition, school attendance and drop-out rates.

1. Introduction

The last three decades have witnessed a significant increase in the share of families who leave their young children in the care of non-relatives, both in developed and developing countries. In the United States, for example, 56 percent of children under age 6 were being cared for by someone other than a parent in 2001, compared to 35 percent of children in 1985 (Baker, Gruber and Milligan, 2008). To support this trend, a large number of countries around the world have implemented policies to subsidize childcare programs. The impact of these policies has been the focus of considerable research seeking to identify the effects on children's development and maternal labor supply. The studies reveal that the effect on children's development strongly depends on childcare quality (see Blau and Currie (2006), Currie (2001) and Ruhm (2004) for a comprehensive review of the literature) and that the increased access to childcare has heterogeneous effects on maternal labor supply (Dhuey, Lamontagne and Tingting, 2016).

While the impact on child development and maternal labor supply is relatively established, less is known about how better access to childcare programs might affect other household members, in particular, other children in the family. To address this issue, this research paper investigates the effect of a Colombian public childcare program on older sisters' time use and educational attainment, operating through a change in their housework responsibilities. Quantifying the spillover effects of childcare programs on older sisters allows a more nuanced measure of the welfare effects of early childhood programs, in the current era of increased investment in early childhood education. In addition, quantifying these effects can improve our understanding of the factors that boost or worsen girls' education.

The direction of the effect of childcare access on older sisters' education is unclear. On one hand, if older sisters were the main caregivers of their young siblings[1], the availability of childcare could liberate their time, allowing them to spend more time in school related work, thus facilitating their educational attainment. On the other hand, if the availability of childcare programs incentivizes female labor supply, mothers could transfer housework responsibilities to older sisters, who now would have less time to devote to their studies.

To answer the question of whether childcare access affects older sisters' education outcomes, I study a public childcare program in Colombia, *Hogares Comunitarios de Bienestar* (HCB). HCB is a nationwide community early childhood program established by the Colombian government in 1986 to provide childcare and food to preschool children. The program has expanded rapidly since its introduction and is currently one of the largest welfare programs in Colombia: there are approximately 80,000 HCB centers across all municipalities in the country and these serve about one million children, from the poorest Colombian families. The cost of the program is significant, approximately $250 million, or almost 0.2% of Colombian GDP, compared to the Head Start program, which in 2004 cost $6.075 billion, or almost 0.05% of the US GDP.

To identify the effect of this childcare program on older sisters' time use and educational attainment, I implement an instrumental variables strategy, where the time needed to travel from the household to the closest HCB center is used as an instrument for participation in HCB. This strategy follows Attanasio, Di Maro and Vera-Hernández (2013) and Card (2005), who also use proxies for the availability and cost of programs as instruments for participation. The main threat to the validity of this empirical strategy is that households, as well as child care centers, tend to be located close to

[1] Studies of children's time allocation have found that older children spend a significant amount of time in the care of younger siblings (Deutsch, 1998) and that the presence of older children in the household is negatively associated with the household choosing outside care (Deutsch, 1998; Fong and Lokshin, 1999).

other facilities like schools, health centers and the town hall. Therefore, the instrument could be capturing the effect of access to education and health facilities, as opposed to access to childcare services. I address this threat by controlling for the time from the household to schools and health centers.

The results indicate that older sisters with young siblings who participate in HCB tend to spend *more* time doing housework tasks. These results would seem to support the hypothesis that better access to childcare programs lead mothers to enter the labor force and transfer their housework responsibilities to their older daughters. This would be consistent with Ilahi (2001) who finds that decreases in local female unemployment rates are related to increases in the time that boys and girls spend on housework in Peru. This change in older sisters' time use, however, does not seem to affect their educational attainment, as no significant effects are found on grade repetition, school attendance or dropping-out rates. It is possible that the allocation of time to housework activities could affect girls' academic achievement, as opposed to educational attainment. This remains an open question for future research.

This paper contributes to the literature in several ways. This is the first paper that assesses the spillover effects of childcare programs on other members of the family. The vast majority of the literature has focused on the effect of childcare programs on the development of participating children[2] or on maternal labor supply[3], with scant attention to other members of the household, such as siblings. The only study on this topic is Currie, Garces and Thomas (2002), who provide suggestive evidence that

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[2] Early Childhood programs such as Head Start, Perry Preschool and Abecedarian have been widely recognized for their positive effects on children development (Deming, 2009; Puma et al., 2012; Schweinhart et al. 2005; Heckman, et al. 2010; Campbell et al., 2001; Heckman et al., 2014).

[3] This literature has found heterogeneous effects of access to childcare on maternal labor supply. The effects depend on the marital status of the mother (Goux and Maurin, 2010; Cascio, 2009; Berlinski and Galiani, 2007), the baseline level of maternal employment (Nollenberger and Rodriguez-Planas, 2015; Calderon, 2014) and the labor market outcome studied - labor force participation or number of hours worked (Dhuey, Lamontagne and Tingting, 2016; Havnes and Mogstad, 2011; Fitzpatrick, 2012).

younger siblings of children who previously participated in Head Start were less likely to commit crimes.

Second, this paper contributes to the literature on educational spillovers between siblings. Previous economics research concentrating on siblings has mainly focused on the intra-family allocation of resources (Becker, 1981), where parental investments on the human capital of children depend on parental preferences regarding inequality between children[4], birth order and the number and gender composition of siblings.[5] Only recently have researchers begun to look at the effect of interactions between siblings, focusing on how older siblings' academic achievement or educational attainment affects the school outcomes of their younger siblings (see Oettinger 2000; Joensen and Nielsen 2013; Adermon 2013; Nicoletti and Rabe, 2014; Qureshi 2015). This paper contributes to this growing literature, by providing evidence of spillovers in the opposite direction (from young siblings to older siblings) that so far have been unexplored.

Finally, this paper provides causal evidence of the factors that determine the time that children spend on activities such us housework. Although the child labor literature has widely studied the determinants of children's time allocation, the discussion has focused on the time that children spend in non-leisure activities such as market labor[6] and schooling, with a limited focus on housework. This omission can understate the amount of time spent on work at home, especially for girls (Mason and Khandker, 1997; Grootaert and Patrinos, 1999; Ilahi 2001)[7]. The few existing studies have identified

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[4] See Behrman et al. (1982), Ermisch and Francesconi (2000), Bernal (2008), and Rammohan and Robertson (2011)

[5] See e.g. Becker and Tomes (1976), Behrman and Taubman (1986), Butcher and Case (1994), Kessler (1991).

[6] Market labor includes working for wages or working in a production process in the household that results in marketable output.

[7] This is particularly important in low and middle-income countries where women invest more time on housework and childcare, compared to men (Kumar and Hotchkiss, 1988; Fafchamps and Quisumbing, 1998; Burda, Hamermesh, and Weil, 2013). The literature on intra-household time allocation in developing countries has mainly investigated how adults allocate their time between income generating activities and housework, and whether this allocation is affected by changes in the wages of the spouse,

correlates of children spending time on housework, such as female unemployment, infant sickness and wealth, but causal evidence is surprisingly scarce (Pitt and Rosenzweig, 1990; Ilahi, 2001; Skoufias, 1993; Galasso, 1999; Grootaert and Patrinos, 1999).

The remainder of this paper is organized as follows. Section 2 describes the HCB childcare program. It then moves to Section 3, which provides a conceptual framework for the spillover effects on older siblings. I then proceed to describe the data in Section 4 and the empirical strategy in Section 5. The results are then presented in Section 6, finishing with the conclusions and discussion in Section 7.

2. The Hogares Comunitarios de Bienestar (HCB) program[8]

The HCB program started its operation in 1986 and is run by the Instituto Colombiano de Bienestar Familiar (ICBF). At the beginning, the ICBF regional office targeted poor neighborhoods and localities and encouraged eligible parents with children aged 0 to 6 to form 'parents associations'. After a few meetings with program officials, the parents associations were registered with the program and elected a *madre comunitaria* (or community mother). This mother should have primary education, a large enough and adequate house and would be certified by the regional office of the ICBF. The *madre comunitaria* (MC) would take care of up to 15 children aged 0 to 6 in her house. She would be also eligible for a subsidized loan to improve the infrastructure at her home to properly attend children, which means having at least one room, a kitchen and a bathroom, according to ICBF.

The length of the care ranges from full-time (8 hours) to part-time (4 hours). However, the sample included in this paper is more likely to have access to part-time centers only. This is because the households in the sample are concentrated in rural areas and 40% of

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access to basic services, agricultural modernization and idiosyncratic shocks; for a comprehensive review of this literature see Ilahi (2000).

[8] This section is a summary of the program description done by the Attanasio et al. (2013).

them live in the Atlantic region, which are geographical locations where part-time centers tend to be concentrated (Bernal and Fernandez, 2013).[9]

Each family pays a small monthly fee, which is used to complement the MC's wage (the rest of her wage comes from a fixed amount paid by the ICBF). The fee is negotiated between the parents' association and the MC and is approved by the local office of the ICBF. The median monthly fee in poor rural areas is 3,800 pesos (approximately $1.5), which corresponds to 1% of the monthly minimum wage as of 2004[10]. The ICBF provides the funds to purchase the food. Children are given lunch, two daily snacks and a nutritional beverage called bienestarina. According to ICBF, the food and beverage provided constitutes 70% of the recommended daily amount of calories.

Eligibility is means tested using a poverty index (SISBEN or *Sistema de Beneficiarios potenciales para programas sociales*), widely used in Colombia to target social policy. Each household is assigned a SISBEN category that ranges from 1 to 6 (1 being the poorest), on the basis of the value of the SISBEN score. This score is constructed using different indicators of economic well-being and is periodically updated by the local authorities. Children are eligible to participate in HCB if they belong to SISBEN 1 and 2.

After the start of the program and its rapid growth, the turnover among the MC was substantial. According to officials of the ICBF, between 10 and 15% of the existing HCBs are relocated in each year, in that a MC ceases to be such and a new one starts to operate it. Moreover, if a household moves to a certain neighborhood, it can normally register its children in an existing HCB, if there are available spaces. It seems that over time, the HCBs have evolved into relatively mobile and informal nurseries and have lost some of the tight connection with the original parents' associations. Nowadays, one

[9] More details about the sample will be presented in section 4.

[10] Calculations based on the 2004 Familias en Accion survey. This survey is described with more detail later in section 5.

parent association is responsible for between 15 and 20 HCB childcare centers. However, MCs need to be certified by the ICBF, must maintain constant contact with it and provide the ICBF, at least in theory, with records of child development and growth.

In rural and isolated areas, an apparently common problem is the difficulty to set up a new HCB because the ICBF does not start a new center unless there are a sufficient number of children who want to attend. On the other hand, in urban areas, the constraint is the number of places available: in many situations HCBs have waiting lists.

3. A conceptual framework

This section describes two simple intuitive models through which HCB might affect the time use and educational attainment of older siblings in the household.

The first model follows the theoretical framework outlined in Qureshi (2015), in which the oldest sister affects the human capital acquisition of her younger brother. Starting with the standard model of human capital investment (Becker, 1964), parents allocate investments allocations among their children, by weighing the benefit of investment against its costs. Older siblings' time can be invested in attaining more schooling or in taking care of their younger siblings. This creates a trade-off, because increasing educational attainment requires more time spent in school, doing homework and traveling to and from the education institute, and, therefore, less time spent with young siblings. As Qureshi (2015) mentions, this tradeoff is most acute when the young sibling is at pre-school age, since he is not yet enrolled in school and spends most of his time at home.

The tradeoff increases the costs of older sisters' education, as the forgone time giving childcare adds to the direct costs of schooling, such as tuition and fees, uniforms, books and supplies, and travel time from the school to home. The introduction of programs like HCB could ameliorate this tradeoff, since the time young siblings spend with their older sisters can be replaced by the time spent in public childcare. Indeed, time spent in

HCB can be an (imperfect) substitute for the time spent with older siblings, since the caregivers in HCB are community mothers, who are required to have at least basic education (equal to five years in Colombia).

According to this model, a decrease in the cost of HCB (e.g. less travel time from the household to the HCB center) could facilitate the educational attainment of older siblings, by increasing the time that the young sibling spends in HCB and increasing the time that the older sibling invests in her own education.

The second model highlights female labor supply as a mechanism through which HCB affects older siblings' time use and education. The availability of childcare might incentivize mothers' participation in the labor force, which can affect children's education purely through and income effect. However, labor force participation of mothers, per se, could impact her children's education through additional channels. First, as Afridi, Mukhopadhyay and Sahoo (2016) point out, mothers are likely to have more alternative uses of their time than fathers – market work, household chores and leisure. If children's time in doing housework substitute for mother's time, then an increase in labor force participation of mothers may lead to a decline in the educational attainment. Second, the higher female earned income may increase mother's bargaining power and attach a greater weight to her preferences in household resource allocation. If mothers prefer to invest more in their children's education, then we should see an improvement in children's education (Blumber, 1988; Thomas, 1990; Hoddinott and Haddad, 1995; Quisumbing and Maluccion, 2003).

4. Empirical Strategy

The aim of this research paper is to estimate the impact of the availability of community childcare centers on the education of older siblings of eligible children who choose to attend them. It is challenging to identify a credible counterfactual that would allow measuring the average educational attainment of eligible children's older siblings in the absence of the program. HCB is widely available and many of the eligible children who do not attend do so by choice, which would likely introduce omitted variable bias problems.

For instance, consider the OLS equation (1) where Y_{ihm} represents educational outcomes of children. The outcomes of interest consist of children's school enrollment (current and previous year), grade repetition, dropping out of school and time use. The time use outcomes include the number of hours spent at school, doing homework, doing housework, working and resting. HCB_h is the treatment variable of interest (younger sibling's participation in the childcare center), measured as having at least one younger sibling in the household attending HCB or, alternatively, the percentage of younger siblings in the household enrolled in HCB. The equation also includes other covariates to control for child (X_{ihm}), household (X_{hm}) and municipality (X_m) characteristics that might affect educational outcomes. Child characteristics include child's age and gender, and an indicator for whether she attended HCB in the past[11]. Household characteristics include mother and head's age and education, household wealth and composition of the household[12]. To control for the level of development of the municipality, the covariates also include the percentage of households with pipe water and sewage connection, and having a hospital in town.

$$Y_{ihm} = \beta_0 + \beta_1 HCB_{hm} + \beta_2 X_{ihm} + \beta_3 X_{hm} + \beta_4 X_m + \varepsilon_{ihm} \qquad (1)$$

Equation (1) suffers from omitted variable bias problems. On one hand, the poorest children attend HCB, possibly introducing a downward bias. This bias would not be avoided by controlling for per capita household income, because income is also affected

[11] This indicator is included because older siblings who were enrolled in HCB in the past might differ in unobserved ways than older siblings who were not enrolled.

[12] I include controls for the composition of the household (e.g. number of persons 0-5 years old, 6-10, …, 40-59, 60 or more), since households with more young children are in higher need for childcare resources, and households with more elders or more non-working youth or adults might be in less need of them.

by childcare access[13]. Moreover, parents who are more aware of the importance of children's nutrition or who are in need of caregivers are more likely to enroll their children in the program, possibly introducing an upward bias. Hence, the direction of the OLS bias is uncertain.

To solve this bias problem, I use an instrumental variable strategy to identify how HCB affects older siblings' education and time use. Specifically, instead of running (1), I estimate a two equation system of the form:

$$HCB_{hm} = \alpha_0 + \alpha_1 time_{hm} + \alpha_2 X_{ihm} + \alpha_3 X_{hm} + \alpha_4 X_m + v_{hm} \qquad (2)$$

$$Y_{ihm} = \beta_0 + \beta_1 HCB_{hm} + \beta_2 X_{ihm} + \beta_3 X_{hm} + \beta_4 X_m + u_{ihm} \qquad (3)$$

by two-stage least squares and where I use time to HCB as an instrument for participation in HCB in equation (2) and, then, use the prediction $\widehat{HCB}$ to estimate the effect of HCB on children's outcome in equation (3).

The validity of the results depends critically on the identification assumptions. These assumptions are that: i) time to the center is a strong predictor of attendance to HCB (the relevance condition) and, ii) time to the center does not affect educational attainment or time use of older siblings, through a channel different than the participation of younger siblings in HCB. (the exogeneity condition) In the next section, I discuss the plausibility of these assumptions, possible threats to validity and how I overcome them.

The validity of time to HCB as an instrument

[13] This is because childcare options influence female labor supply and, hence, could increase household income. The omitted variable bias problem is partially corrected when controlling for household wealth and parental education. However, this does not control for income volatility, which is a source of uncertainty and a possible driver of poverty. This omitted variable bias problem had already been identified by Attanasio et al. (2013) and Bernal and Fernandez (2013).

The instrumental variables literature has used measures of distance/time to a center as proxies for the cost of participation in a program. For example, Card (2005) uses distance to college as an instrument for college attendance to examine the returns to higher education. Another example is Attanasio et al. (2007, 2013), who uses time to HCB as an instrument for HCB participation, in order to measure the effect of HCB on female labor supply, children's nutrition status and children's medium term educational attainment.

In the context of this paper, time from the household to the HCB is negatively correlated with the treatment, since children who take long to get to a HCB are less likely to attend the childcare centers. Time can be regarded as a strong instrument because it represents an important constraint for children's attendance in rural areas, as indicated by ICBF officials (Attanasio et al. 2010). This affirmation is supported by the data. When parents of children 2-4 years old who do not attend HCB were asked about the reasons for not doing so, nearly 25% answered that it takes them long to get to the closest HCB or that there is no HCB facility nearby (Table 23).

Regarding the exogeneity restriction, the main threat to validity is that wealthier households may choose to live close to schools, health centers and the town center. As HCB centers also tend to be located closer to these amenities, we would expect wealthier households to live closer to HCB childcare centers. Hence, any apparent positive effect of HCB on older siblings' time use and education may instead be the result of families' wealth or easier access to a school. I am able to assess this threat because the data used in this paper has information on the location of households. The data records whether the household lives in the center of the town, and the time to the town hall, health center and school (section 5 will describe the data with more detail). This allows me to control for the time to get to these amenities.

Another threat to validity is that families who care about their young children's nutrition or need support in giving care to them choose to live close to HCB centers or, alternatively, advocate for opening new centers close to them. If this were true, any positive effect of HCB on the time use and education of older siblings would reflect parents' behavior. Regarding endogenous mobility, Attanasio et al. (2013) provides evidence that households do not move with the main purpose of living closer to a HCB, probably due to the high turnover of these childcare centers. The households that changed location were asked the reason for moving. Although "moving to a closer HCB" was explicitly listed as an explanation, almost no households chose it.[14]

In addition, the evolution of the HCB program makes it unlikely that nowadays parents would be able to put pressure on officials to open new centers. After the start of the program and its rapid growth, the turnover among the MCs was substantial and the link between the original parent association and the location of HCB centers was weakened. Hence, it would be difficult for parents to put pressure on the ICBF officials or to predict the location of new HCB centers. More likely, however, is that local governments advocate for having more HCB centers in town. In this case the instrument could also be endogenous, as any positive effect of HCB on children's time use and educational attainment could reflect other policies that the department (state) governors are implementing, as they focus on children's welfare policies (among them education). To assess this, I check the robustness of the results to the inclusion of fixed effects at the departmental level, and find no difference with the main results.

Another possible threat is that the program targets poor towns. In this case, mostly poor regions would benefit, which would likely bias downward the results. I account for this by controlling for variables that proxy development at the municipality level, like the percentage of households with pipe water and a sewage connection and the existence of a hospital in town.

[14] These are households who moved between two consecutive waves of the survey used in this paper, but were found and interviewed after moving. Section 5 will describe the survey in more detail.

An additional possible source of bias is that the measure of time is self-reported and depends on the transport means of the household. Wealthier households who have a car would perceive themselves as living closer to the HCB, compared to households who live in the same are but have no car. This could cause omitted variable bias, since having a car is a proxy for wealth and as such is negatively correlated with attendance to HCB (since poorer households participate) and positively correlated with educational attainment. I assess this threat by controlling for a household wealth index in the regressions. Although the wealth index does not include information on car ownership (because the survey does not ask about it), it does include information on ownership of other transport means such as a bike, motorcycles or boats, which are also proxies of wealth.

A last threat is that parents' preferences between siblings could also affect the time use and education of older siblings. The parents might have a preference for the younger child, which would make them invest more in her, compared to the older sibling. For instance, they could move closer to HCB so the young child can participate, without doing any comparable investment in the older. This would bias down the results, since the investment in the older sibling is low relative to the investments in the young one. However, as was mentioned before, households who moved between the first and second wave were identified, and when asked about the reasons for moving, they did not choose 'to be closer to a HCB center' as an answer.

Relation of the instrument with other covariates

The estimates of the effects of HCB on older siblings would be inconsistent if the instruments would be correlated with unobserved determinants of children's education and time use. While I cannot compute the correlation with unobserved variables, I can try to assess how realistic the assumption is by analyzing the relation between time to HCB and observed determinants of children's education and time use. If I find that time is correlated with many observed variables, it is highly unlikely that it would not be correlated with unobserved characteristics.

Following Attanasio et al. (2013), I estimate regressions with the instrument at the left-hand side and other socioeconomic characteristics at the right-hand side. This exercise measures the correlation of the instrument with observed determinants of children's education and time use, conditional on other socioeconomic characteristics. The results are reported in Table 24. As column 1 reveals, wealthier households tend to live closer to HCB. Specifically, families whose wealth index is one standard deviation above the sample average live 4 minutes closer to HCB and this relation is significant at the 1% level. This could bias upwards the results, because wealthier households could also choose to live closer to other amenities, which could improve older siblings' education. With the purpose of correcting this bias, I include variables for the location of the household, such as time to the school, health center and town hall, and whether the household lives in the center of the town. As shown in Column 2, once these controls are added, the correlation between household wealth and time to HCB reduces to half its initial size. Conditional on time to other amenities, households whose wealth index is one standard deviation above average live only 2 minutes closer to HCB than households on the average, and this number is distinguishable from zero only at the 10% of significance. Other variables such as child gender and age, mother/head age and education and composition of the household are uncorrelated with the instrument, conditional on the wealth index.

It is worth noting that some of the municipality variables are correlated with time to HCB. The percentage of households with pipe water service is positively correlated with time to HCB, while the percentage with a sewage connection is negatively correlated with it, even after controlling for the average time to amenities in the municipality (Column 2). The opposite correlations are contradictory, since both variables are indicators of the level of development of the municipality and, as such, would be expected to have the same sign. This could be the result of the interaction of two different factors. First, the ICBF (the public institution in charge of HCB) has determined that childcare centers should have a minimum of sanitary and infrastructure conditions to serve as HCB, so the municipalities were the program operates should have a minimum level of development. On the other hand, the program targets poor households, and as such is likely to operate in less developed municipalities. Together, these two explanations could indicate that HCB operates in rural areas with a medium level of development. However, the sign of the bias that this causes is unclear.

5. Data

The data used comes from a survey originally collected to evaluate *Familias en Accion* (FeA), a Conditional Cash Transfer (CCT) program.[15] The households included in the survey had to satisfy the FeA eligibility criteria, which consist of living in relatively small towns with sufficient education and health infrastructure[16], classifying in the lowest socio-economic category[17] and having children between 0 and 17 years old. This implies that the FeA sample is representative of the poorest households in small towns.

The FeA survey includes information on a variety of municipal and demographic variables, as well as current and past attendance of each child to HCB. In particular, it asks about the number of months the child participated in HCB for each year of her life, as well as the travel time to the closest HCB center.

To avoid contamination with the FeA program, this paper uses the baseline survey (collected in summer 2002) and includes only towns where FeA had not yet been

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[15] FeA consists of a cash transfer to families, conditional on school attendance by children ages 7-17. An additional basic nutrition subsidy is provided to families with children under 6 years, conditional on mothers taking their children to growth and development check-ups.

[16] The towns targeted by FeA had a population less than 100,000, were no department (state) capitals, and had sufficient education and health infrastructure, as a well as a bank.

[17] The socio-economic categories are constructed based on a welfare index (SISBEN), which is used by the Colombian government to target public social programs. The index is based on information systems that record assets, income and characteristics of the household.

implemented.[18] The sample includes 65 municipalities, which includes 4,098 children between 0-6 years old and 9,178 children aged 7-17, of whom 4,452 have young siblings below age 6. However, the estimation sample only includes 2,612 of them due to missing variables.

The main treatment variable is participation of young siblings in HCB. I consider two alternative definitions of participation in HCB: having at least one young sibling participating in HCB and the percentage of siblings below 6 years old participating.[19]

The instrument for participation in HCB is the travel time from the household to the closest HCB center, which is a proxy for cost of participation.[20] The question is framed in terms of time to travel (minutes), instead of distance to travel (km or miles). This is a feature typical of the Colombian household surveys, which takes into account that the extensive mountain system existent in Colombia greatly affects travel time. The large chain of mountains challenges the construction of transport infrastructure, which means that one mile of distance could represent a 20 minute-walk for a household that lives in a plain valley, compared to one hour-walk for a household that lives in the mountains.

The regressions also include household socio-demographic characteristics that come from the survey. These consist of the child's age and gender, the mother's and the head of the household's age and gender,[21] and the composition of the household.[22] A wealth index is

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[18] Other waves were collected in July-November 2003, December 2005-March 2006 and December 2011-January 2012. FeA was phased in and by 2006 all municipalities in the survey had received it.

[19] This measure of participation in HCB is contemporaneous, because it does not capture past participation of young siblings in HCB. Past participation could have affected past school enrollment of older siblings and, therefore, current school enrollment. I do not consider past participation as an alternative measure because there would be no instrument for it; the FeA survey does not ask about past travel time to the closest HCB.

[20] For households with participating children, this travel time variable records the time to the HCB center, while for those households with no participating children, it records the time to the closest HCB.

[21] I cannot identify who is the mother of every child in the household. However, I can identify the mother of every child below 0-6 years. I assume that the mother of every child younger than 6 is the same as the mother of every other child in the house. Whenever there are two mothers of children below 6 (63 households), I choose the older of them as the mother.

also added, constructed based on the household's assets and appliances information[23]. This index is standardized around the sample mean.

Other variables include the travel time to the town hall and to the closest school and health center, as well as indicators for households living in the center of the town or in isolated rural areas. As explained in the previous section, these variables allow controlling for location factors that might influence older siblings' education, through channels different than the access to HCB centers.

The municipality level controls consist of variables that measure local economic development, such as having a hospital, and the percentage of households with pipe water and sewage connection.[24] Other variables include the geographic region of the municipality, which account for the differences in economic development across the Colombian territory.[25]

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[22] It includes variables for the number of persons in the household in a specific age range: persons 0-5 years old, 6-10, 11-15, 16-17, 18-39, 40-59, and 60 years old or more.

[23] Using principal components, I consolidate into a unique index information on ownership of a house, refrigerator, sewing machine, radio, bike, motorcycle, fan, blender, boat and animals. The index consists of the first principal component, which explains 21% of the total variance.

[24] These variables come from the 2005 census, except for the existence of a hospital, which is reported by the mayor and included in the household survey.

[25] The survey defines four geographical regions: Atlantic, Andean, Pacific and Coffee Zone.

Descriptive Statistics

The main characteristics of the estimation sample are shown in Table 25. The sample consists of 2,612 children between 7-17 years old, who have at least one young sibling (below 6 years old) living in the household.

Although the FeA survey is representative of rural areas in Colombia, 51% of the children in the sample live in the center of the town rather than dispersed in the countryside. Most mothers (67%) have not finished primary education, as well as most heads of the household (70%). The households in the sample are big, with 7.2 members on average. Of these, 4.5 are children below 15 years, who are almost evenly distributed in age range: 1.4 in the 0-5 years range, 1.7 in the 6-10 range and 1.3 who are 11-15. Only a few households have elders living with them (0.13 persons 60 or more years old), who could be potential caregivers of the young children.

In terms of the degree of development of the municipalities included in the sample, 71% of them have access to a hospital, while only 66% (38%) of the households have access to pipe water (sewage connection). The municipalities in our sample are heavily located in the Atlantic region (44%), followed by the Andean and Coffee regions (22% each) and the Pacific region (13%).

Half of the older siblings have attended HCB in the past, while one-third of them have at least one sibling currently attending (29%). On average, 24% of their young siblings are in HCB. The households live closer to schools (14 minutes) than to HCB (24 minutes), but farther away from health centers (44 minutes) and town halls (56 minutes).

Education in Colombia is mandatory until the 9th grade (roughly 15 years old). Since the sample includes children until 17, we do not observe full enrollment. School enrollment rates of children 17 and younger during the current and last year are 80% and 83%, respectively. Of the 80% enrolled the year before, about a sixth failed and few dropped out. Prolonged self-reported absences are not common, with only 3% of the parents reporting their children being absent last year during more than 20 days.[26] Children spend almost 1 hour doing housework (which can include caregiving), which is non- negligible if it's compared with the amount of time spent doing homework (1.5 hours).

Thirty seven percent of the children's mothers are currently working in the market (757 mothers).[27] Working mothers labor for 37 hours per week and worked on average 3.6 months during the last 12 months.

Figure 14 presents the distribution of age in the sample. Children's age mostly falls in the 7-10 years old range (1534 observations) and, to a lesser extent, in the 11-14 years range (853). There are significantly fewer children in the 15-17 years range (234), as youth in low income families leave their households early.

Figure 15 presents participation of younger siblings in HCB, differentiating by age range and gender of the older sibling. Overall, girls and boys do not have younger siblings participating in the program at a different rate.[28] Accordingly, girls do not report to live closer to HCB than boys (Figure 16).

Although the HCB program is targeted to the poorest families in Colombia, it is not the poorest of the poor who participate. Figure 17 shows the participation in HCB by income quintile within the sample, as defined by the income per capita in the

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[26] This number might be underestimated, because parents have the incentive to underreport absences, to get the FeA cash transfer.

[27] A mother is considered currently working in the market if she reported spending most of her time working during the previous week.

[28] The exception is girls in the 15-17 years old range, who seem to have more young siblings attending HCB (30%), compared to boys (21%). However, this difference is not statistically different from zero, probably because the number of observations in this age range is very small.

household.[29] A big portion of the participation comes from households in the 2nd and 3rd quintiles, as compared to the 1st quintile. One possible reason for this is that very poor households might not be able to pay for the fee and are more likely to have multiple generations living together as well, which increases the number of possible caregivers at home. On the other hand, children in households in the 4th and 5th quintiles attend at lower rates, perhaps because they are able to pay for other private childcare options[30].

Program uptake also seems to be higher in poorer municipalities, which was expected given that HCB targets poor families. As Figure 18 reveals, the percentage of children with younger siblings in HCB is positively correlated with the percentage of poor households in the municipality.

Participation in the program is strongly correlated with the time to HCB. As Table 26 shows, children with younger siblings attending HCB live on average 18 min closer than children with no young siblings attending. This relation also holds for other points in the time distribution, as shown in the Table.

6. Results

First Stage Estimations

The first stage equation (2) predicts participation of younger siblings in the program as a function of time to HCB. The unit of observation is older siblings, e.g., children 7-17 years old who have siblings 0-6 years old living at home. As mentioned before, participation in HCB is measured as i) having at least one younger sibling in HCB and ii) percentage of younger siblings in HCB. The instrumental variable time to HCB is included as a linear term, together with other covariates (mentioned in section 4). The

[29] The quintiles are ascending in income.

[30] I do not have information about the private childcare options for low-income families. However, anecdotal evidence suggests that working mothers who have no other caregivers at home pay other non-working mothers in the neighborhood to take care of their children.

first stage is estimated first using the entire sample, and then on females separate from males, since I am interested in heterogeneous effects of HCB by gender.

According to Table 27, time to HCB is a strong predictor of participation in the program. Columns 1 and 2 reveal that living 100 minutes closer to HCB increases participation in the program approximately by 16 percentage points, for both measures of participation considered. The F-statistics that result from performing a test of the significance of the instrument are 10.9 if participation is measured as having at least one young sibling in HCB and 12.6 if it is measured as the percentage of young siblings in HCB. These two statistics are above the 10-threshold recommended by Stock and Yogo (2005). The strength of time as an instrument in the full sample seems to be completely driven by girls. As columns 3 and 4 reveal, the F-statistics for the female sample are much higher (35 and 30, respectively), as opposed to the male sample with F-statistics 4.8 and 6.4, which is a sign of a weak instrument problem for older brothers.

Hence, time to HCB is a strong instrument for older sisters, but not for older brothers, which could be interpreted in two ways. The first is that families of females are intrinsically different than families of males. Table 25, which presents summary statistics by gender, reveals that this is unlikely to be the case. Observed characteristics of girls' families are not very different than boys' families. More plausible is that gender roles in Colombia are important when considering time to HCB as a constraint. For instance, older sisters could be considered as an option for taking care of young siblings, while older brothers are not. Hence, when facing the decision to enroll a young child in HCB, girls' mothers weight the cost of enrolling the child (e.g., traveling a long time) vs. leaving him to the care of the oldest sister. Boy's mothers might not face this dilemma since older brothers would not be considered possible caregivers.

Effect of HCB on older siblings' education and time use

Table 28 presents the OLS and IV estimates using the entire sample (both females and males). The dependent variables measure educational attainment (Panel A) and children's time use (Panel B). As mentioned in section 4, I include a large set of covariates at the individual, household and municipality level, which include time from the household to the closest school, health center and town hall. The regressions also include region dummies, and all standard errors are clustered at the municipality level.

Panel A reveals that the OLS coefficients in the education equation have no consistent direction (positive or negative). For instance, children with young siblings attending HCB are 3% more likely to have been enrolled in school the year before, but are as well 1.6% and 2.4% more likely to have dropped out or to have been absent 20-40 days, respectively. These contradictory results could be the result of the multiple directions the OLS bias could take (see section 4 for a detailed discussion of possible biases).

The IV estimates of the educational attainment regressions are not conclusive either. The signs of some coefficients reveal that children with young siblings attending HCB tend to have *worse* educational outcomes than children who do not, while other coefficients point to the opposite direction. For example, those with younger siblings in HCB are 3% less likely to be currently enrolled, 16% less likely to have been enrolled last year, 9% less likely to have successfully completed the last grade and 3% likely to have dropped out. These coefficients are not statistically different from zero and conflict with other results, like being less likely to fail (12%) or less likely to be absent 20-40 days (8%).

Similarly, Panel B shows that the effect of HCB on children's time use is imprecisely estimated. The IV coefficients on time spent working, doing homework or housework are different depending on the period recalled (the day before the interview or during extra school hours in general) and they are not statistically different than zero. It is worth noting that OLS reveals that children with younger siblings attending HCB tend to do more housework than children who do not. This difference is statistically different from zero at the 90% confidence level, for both periods considered (day before or extra school hours) and for both measures of participation (having any young sibling in HCB or the percentage of young siblings in HCB).

Effect of HCB on older sisters vs. older brothers

Table 29 presents the IV estimates for older sisters and reveals that the results on the education equations are inconclusive, following the pattern with the full sample. For instance, older sisters with younger siblings in HCB were more likely to be enrolled the current year and less likely to have failed or dropped out, but, they were also less likely to be enrolled the year before and more likely to be absent more than 20 days. Furthermore, none of these coefficients are statistically different from zero.

However, Table 7 does show a salient feature of older sisters' time use. The OLS equation shows that girls with young siblings in HCB do 0.2 more hours (12 minutes) of housework than sisters who do not. When I include time to HCB as an instrument, this coefficient significantly increases to 1.1 more hours for both measures of participation considered (having any young sibling in HCB or the percentage of siblings in HCB). This is a large effect, considering that the sample average number of hours that older sisters spend doing housework is 1.5 hours (Table 3). Hence, the evidence suggests that participation of young siblings in HCB causes older sisters to spend more time doing housework.

Table 30 presents the IV estimates for older brothers for completeness purposes. These results should not be given much consideration, since time to HCB is a weak instrument for the male sample and no inference should be made if the instrument does not induce participation in the program for this subsample.

Why does HCB induce girls to spend more time doing housework?

One possible explanation for the results is the effect of HCB on female labor supply. Mothers who were previously taking care of the young children (full-time or part-time) would be able to work or to increase their hours of work when the program becomes more accessible to them. They could then transfer to their older daughters some of their housework duties.

Table 31 presents the effect of HCB on female labor supply, using also time to HCB as an instrument and including the same set of covariates as the education and time use equations. The unit of observation is still children 7-17 with siblings aged 0-6, but it includes only children for whom the mother's labor information is available, which decreases the sample size to roughly 2000 observations. Since the unit of observation is the child and not the mother, these regressions implicitly weight each mother by the number of children between 7 and 17 years in the household.

Unfortunately, the first stage is sensitive to the number of observations. The F-statistics were above the 10-threshold when all children 7-17 were incorporated, but when only children with information on maternal labor supply are included, the numbers decrease to 6.3, making time to HCB a weak instrument for this subsample. Thus, the second stage results should be interpreted with caution, since a weak instrument biases the IV estimates in the direction of the OLS estimates. As mentioned in section 4, there are two possible directions for the OLS bias: downwards, since mostly poor households enroll children in HCB, and also upwards, since those parents more aware of the importance of children's nutrition are the ones who enroll them in the program.

Having noted the weakness of time as an instrument for this subsample, I proceed to describe and interpret the results. The evidence indicates that mothers who have any young child in HCB are 85 percentage points more likely to be working, compared to mothers with no child in HCB. On average, they work 41 more hours per week and 9 more months per year. These coefficients are too large to be plausible. The results are similar when I only use the sample of working mothers, which estimates the parameter in the extensive margin (and avoids imputing zero hours and months of work to non-working mothers). Although this change in the sample decreases the size of the coefficients, their magnitudes are still large and implausible. When HCB becomes available, working mothers increase their time working to 30 more hours per week and 2.4 more months per year. However, only the first of these numbers is significant at the 10% level, probably because the sample size reduces abruptly to 734 observations.

7. Conclusions and Discussion

This paper uses one of the largest welfare programs in Colombia, Hogares Comunitarios de Bienestar (HCB), to measure the spillover effects of childcare programs on older siblings in the family. I attempt to identify the effect of having a younger sibling in HCB on older siblings' educational attainment. The mechanism proposed is time use, since older children might be the caregivers of younger ones, preventing them from spending more time on school-related duties. This effect is expected to be stronger for older sisters than for older brothers, given the gender roles characteristic of Colombia and other Latin American countries.

Since the OLS estimates are likely to suffer from omitted variable bias, I use time to HCB as an instrument for participation in the program. Time is a proxy for the availability and cost of the program, so it should induce the participation of a portion of the population. The main threat to the validity of the instrument is that wealthier households tend to locate close to other amenities like schools, health centers and the town center. Since HCB centers also locate close to these facilities, this would induce a correlation between the instrument and the outcomes, violating the exclusion restriction. However, the time from households to schools, health centers and town hall are observed and are included in the regressions, controlling for this endogenous relation.

The results reveal that females with young siblings attending HCB spend *more* time doing housework than females with no siblings in HCB. However, this deviation of older sisters' time towards housework does not seem to affect their educational attainment, as the results of the effects on school attendance, drop-out and grade repetition are not conclusive. Although it would be desirable to compare these results to those of older brothers, it is not possible to do so with this sample, because time to HCB is a weak instrument for attendance to HCB in the sample of boys with young siblings ages 0-6.

The effect of HCB on older sisters' time allocation could be explained by the effect of the program on female labor supply. The availability of the program could facilitate the participation of mothers on the labor force, who in turn would need additional support from their older daughters to perform housework. Indeed, the literature on the effects of HCB on household well-being points in this direction. Attanasio and Vera (2007) find that when HCB is available, mothers likelihood to be employed increases by 37 percentage points and their number of hours worked goes up by 75 per month. Given these effects on female labor supply. However, it is not possible to replicate these results with my estimation sample. When restricting the sample to households with a record of maternal labor supply, and that have children in both the 0-6 and 7-17 age range, the sample size reduces drastically. This reduction makes time to HCB a weak instrument for participation, limiting the conclusions that can be drawn from the second stage estimations. Therefore, further research is needed to identify the interaction between participation of a younger sibling in HCB and the female labor supply, and the effect of this interaction on the amount of older sisters' housework.

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9. Figures and Tables

Table 23 Reasons for not attending HCB

	Age 0-1	**Age 2-4**	**Age 5-6**
No HC facility	9.6%	12.9%	7.6%
Too far	7.8%	10.4%	5.6%
Cannot afford fee	6.3%	9.9%	3.5%
Available caregiver at home	68.5%	34.8%	18.0%
The child is studying	0.1%	24.0%	68.2%
Age	8.1%	3.7%	2.6%
Other	5.1%	14.7%	26.9%

Number of observations: 3,954 eligible children not attending. Data from FeA first wave

Table 24 Relation of time to HCB with covariates

	Time to HCB (1)	Time to HCB (2)
Female	-0.0151	-0.00519
	(0.0185)	(0.0171)
Child age	-0.0223	0.00766
	(0.0284)	(0.0261)
Child age squared	0.00124	-4.67e-05
	(0.00131)	(0.00113)
ln(head age)	-0.0112	-0.0417
	(0.0819)	(0.0729)
ln(mother age)	-0.108	-0.0913
	(0.254)	(0.240)
Mother no primary	0.0543	-0.0118
	(0.0356)	(0.0304)
Mother primary	0.0190	0.00441
	(0.0379)	(0.0356)
Head no primary	0.0401	-0.0291
	(0.0419)	(0.0412)
Head primary	0.0645	0.0159
	(0.0534)	(0.0470)
Wealth index (standardized)	-0.0448***	-0.0225*
	(0.0160)	(0.0114)
Nchildren 0-5	0.0174	0.00642
	(0.0197)	(0.0194)
Npersons 6-10	0.0253	0.0166
	(0.0236)	(0.0200)
Nchildren 11-15	0.0100	0.00894
	(0.0212)	(0.0206)
Nchildren 16-17	-0.0368	-0.0151
	(0.0389)	(0.0370)
Npersons 18-39	-0.00680	-0.000343
	(0.00825)	(0.00865)
Npersons 40-59	0.0323	0.0249
	(0.0286)	(0.0309)
Npersons 60 or more	0.0140	0.0143
	(0.0302)	(0.0300)
Hospital in town	0.0531	0.0321
	(0.0660)	(0.0448)
% households pipe water	0.187	0.209**
	(0.142)	(0.104)
% households sewage connection	-0.380***	-0.207***
	(0.115)	(0.0754)
Time to health center (minutes divided by 100)		0.141**
		(0.0698)

Table 24 Relation of time to HCB with covariates (cont)

Time to school (minutes divided by 100)		0.308*
		(0.172)
Time to town hall (minutes divided by 100)		-0.00709
		(0.0283)
Lives in the center of the town		-0.153***
		(0.0412)
Av. Time health center		0.0218
		(0.127)
Av. Time to school		1.110**
		(0.520)
Av. Time town hall		-0.0432
		(0.0952)
Atlantic region	-0.399***	-0.270***
	(0.0993)	(0.0690)
Andean region	0	0
	(0)	(0)
Coffee region	-0.0794	-0.0702
	(0.118)	(0.0743)
Pacific region	-0.366***	-0.296***
	(0.125)	(0.0845)
Constant	0.281	-0.0593
	(0.279)	(0.213)
Nobservations	2,621	2,621
Rsquared	0.157	0.311

Table 25 Summary statistics

Panel A. Sociodemographic characteristics

Variable	All Mean	All Std. Dev.	All Nobs	Females Mean	Females Std. Dev.	Females Nobs	Males Mean	Males Std. Dev.	Males Nobs
Child characteristics									
Female	0.46	0.50	2621	1.00	0.00	1211	0.00	0.00	1410
Child age	10.27	2.66	2621	10.20	2.59	1211	10.34	2.72	1410
Order	2.41	1.47	2578	2.39	1.43	1187	2.44	1.51	1391
At least one young sibling in HCB	0.29	0.45	2621	0.27	0.44	1211	0.31	0.46	1410
% young siblings in HCB	0.24	0.39	2621	0.23	0.39	1211	0.25	0.40	1410
Attended HCB past	0.50	0.50	2621	0.50	0.50	1211	0.50	0.50	1410
Household characteristics									
Head age	0.40	0.09	2621	0.40	0.09	1211	0.40	0.09	1410
Mother age	0.35	0.07	2621	0.34	0.07	1211	0.35	0.07	1410
Mother no primary	0.67	0.47	2621	0.66	0.47	1211	0.69	0.46	1410
Mother primary	0.29	0.45	2621	0.30	0.46	1211	0.28	0.45	1410
Mother secondary	0.04	0.20	2621	0.04	0.20	1211	0.04	0.19	1410
Head no primary	0.70	0.46	2621	0.67	0.47	1211	0.72	0.45	1410
Head primary	0.25	0.43	2621	0.27	0.45	1211	0.23	0.42	1410
Head secondary	0.05	0.21	2621	0.05	0.22	1211	0.04	0.21	1410
Wealth index (standardized)	-0.06	0.93	2621	-0.06	0.93	1211	-0.07	0.92	1410
Household size	7.29	2.34	2621	7.18	2.32	1211	7.39	2.35	1410
Nchildren 0-5	1.47	0.93	2621	1.43	0.93	1211	1.50	0.94	1410
Npersons 6-10	1.75	0.95	2621	1.73	0.95	1211	1.76	0.95	1410
Nchildren 11-15	1.33	1.04	2621	1.28	1.03	1211	1.36	1.04	1410
Nchildren 16-17	0.27	0.51	2621	0.26	0.50	1211	0.28	0.51	1410
Npersons 18-39	1.69	0.96	2621	1.70	0.95	1211	1.69	0.96	1410
Npersons 40-59	0.66	0.77	2621	0.64	0.75	1211	0.67	0.78	1410
Npersons 60 or more	0.13	0.39	2621	0.13	0.39	1211	0.13	0.39	1410
Municipality characteristics									
Hospital in town	0.71	0.46	2621	0.70	0.46	1211	0.71	0.45	1410
% households pipe water	0.66	0.23	2621	0.67	0.23	1211	0.66	0.23	1410
% households sewage connection	0.38	0.25	2621	0.38	0.26	1211	0.38	0.25	1410
Atlantic region	0.42	0.49	2621	0.41	0.49	1211	0.44	0.50	1410
Andean region	0.22	0.42	2621	0.24	0.43	1211	0.21	0.41	1410
Coffee region	0.22	0.41	2621	0.21	0.40	1211	0.23	0.42	1410
Pacific region	0.13	0.34	2621	0.14	0.35	1211	0.12	0.33	1410

Table 25 Summary Statistics(cont)

Panel B. Time variables

Variable	All Mean	All Std. Dev.	All Nobs	Females Mean	Females Std. Dev.	Females Nobs	Males Mean	Males Std. Dev.	Males Nobs
Time to HCB (minutes divided by 100)	0.24	0.40	2621	0.24	0.38	1211	0.25	0.41	1410
Time to health center (minutes divided by 100)	0.44	0.63	2621	0.41	0.53	1211	0.47	0.70	1410
Time to school (minutes divided by 100)	0.14	0.15	2621	0.14	0.15	1211	0.14	0.15	1410
Time to town hall (minutes divided by 100)	0.56	0.81	2621	0.53	0.76	1211	0.59	0.85	1410
Lives center of the town	0.51	0.50	2621	0.52	0.50	1211	0.51	0.50	1410
Av. Time health center	0.45	0.33	2621	0.44	0.32	1211	0.46	0.33	1410
Av. Time to school	0.14	0.06	2621	0.14	0.06	1211	0.14	0.06	1410
Av. Time town hall	0.58	0.44	2621	0.56	0.43	1211	0.59	0.44	1410

Panel C. Outcomes

Variable	All Mean	All Std. Dev.	All Nobs	Females Mean	Females Std. Dev.	Females Nobs	Males Mean	Males Std. Dev.	Males Nobs
Child education									
Currently enrolled	0.83	0.38	2621	0.86	0.34	1211	0.80	0.40	1410
Enrolled	0.80	0.40	2621	0.83	0.38	1211	0.78	0.42	1410
Passed	0.64	0.48	2621	0.69	0.46	1211	0.60	0.49	1410
Failed	0.13	0.34	2621	0.11	0.32	1211	0.15	0.35	1410
Dropped out	0.02	0.15	2621	0.02	0.13	1211	0.03	0.16	1410
Absent more 20 days	0.03	0.18	2621	0.02	0.15	1211	0.04	0.20	1410
Absent 40+ days	0.01	0.12	2621	0.01	0.09	1211	0.02	0.14	1410
Child time use									
N. hours work day before	0.51	1.85	2621	0.23	1.17	1211	0.76	2.25	1410
N. hours at school day before	1.54	2.50	2621	1.60	2.52	1211	1.49	2.47	1410
N. hours homework day before	0.35	0.83	2621	0.38	0.84	1211	0.33	0.81	1410
N. hours housework day before	0.55	1.27	2621	0.76	1.54	1211	0.36	0.94	1410
N. hours housework extra school	0.11	0.64	2171	0.06	0.48	1044	0.16	0.75	1127
N. hours housework extra school	0.97	0.98	2172	1.11	1.00	1045	0.85	0.93	1127
N. hours homework extra school	1.45	0.73	2172	1.49	0.71	1045	1.41	0.74	1127
N. hours housework extra school	2.86	1.44	2172	2.70	1.41	1045	3.01	1.46	1127
Female labor									
Mother working	0.37	0.48	2054	0.39	0.49	948	0.35	0.48	1106
N. hours work/week (conditional)	36.95	19.41	737	37.12	18.82	354	36.80	19.96	383
N. hours work/week (unconditional)	13.32	21.19	2034	14.00	21.38	935	12.75	21.02	1099
Nmonths work (conditional)	9.88	3.64	751	9.90	3.51	364	9.85	3.77	387
Nmonths work (unconditional)	3.62	5.25	2050	3.81	5.29	946	3.45	5.20	1104

Table 26 Distribution of time to HCB

	Time to HCB (mins)	
	All children	Children with at least one young sibling HCB
25th percentile	3	3
Median	7	5
Mean	26	8
75th percentile	30	10

Table 27 First stage regressions

	All the sample		Females		Males	
	At least one young sibling HCB	% young siblings HCB	At least one young sibling HCB	% young siblings HCB	At least one young sibling HCB	% young siblings HCB
	(1)	(2)	(3)	(4)	(5)	(6)
Time to HCB	-0.167***	-0.151***	-0.190***	-0.172***	-0.154**	-0.139**
	(0.0504)	(0.0424)	(0.0322)	(0.0315)	(0.0698)	(0.0551)
Observations	2,612	2,612	1,207	1,207	1,405	1,405
R-squared	0.252	0.223	0.278	0.244	0.249	0.225
F-statistic	10.9	12.6	34.9	29.7	4.8	6.4

Notes: Standard errors in parentheses are clustered at the municipality level. The units for time to HCB are minutes divided by 100. The regressions include child and household characteristics such as child's age and gender, the mother's and the head of the household's age and gender, and indicators for the composition of the household (number of persons 0-5 years old, 6-10,11-15, 16-17, 18-39, 40-59 and 60 years old or more). A household wealth index is also included, constructed based on the household's assets and appliances information. Municipality characteristics include having a hospital in town, percentage of households with pipe water and percentage with sewage connection. I also control for household location variables, such as living in the center of the town and time to the health center, the school and the town hall. Other variables include department (state) fixed effects and indicators for the geographic region in Colombia (four in total).

Table 28 Effect of HCB on older siblings' education and time use

Panel A. Effect on education outcomes

	Enrolled (this year)	Enrolled (last year)	Passed (last year)	Failed (last year)	Drop out (last year)	Absent 20-40 days (last year)	Absent 40+ days (last year)
				OLS			
Any young sibling in HCB	0.0161	0.0330	0.0172	0.00264	0.0160	0.0243*	0.0131
	(0.0204)	(0.0256)	(0.0467)	(0.0329)	(0.0103)	(0.0144)	(0.00831)
Pct young siblings HCB	0.000753	0.0211	0.0183	-0.0154	0.0217*	0.0270	0.0169
	(0.0244)	(0.0335)	(0.0543)	(0.0380)	(0.0129)	(0.0167)	(0.0107)
				IV			
Any young sibling in HCB	-0.0326	-0.169	-0.0979	-0.126	0.0290	-0.0869	0.00298
	(0.186)	(0.211)	(0.192)	(0.198)	(0.0736)	(0.137)	(0.0664)
Pct young siblings HCB	-0.0361	-0.187	-0.108	-0.139	0.0321	-0.0961	0.00329
	(0.206)	(0.237)	(0.212)	(0.224)	(0.0826)	(0.150)	(0.0736)
Nobs	2,612	2,612	2,612	2,612	2,612	2,612	2,612

Table 28 Effect of HCB on older siblings' education and time use (cont)

Panel B. Effect on Time Use

	Day before				Extra-school hours			
	N. hours work	N. hours school	N. hours homework	N. hours housework	N. hours work	N. hours housework	N. hours homework	N. hours rest
	OLS				*OLS*			
Any young sibling in HCB	0.0924	-0.0192	0.000126	0.114*	0.0330	0.128*	-0.0423	0.0105
	(0.150)	(0.112)	(0.0374)	(0.0681)	(0.0536)	(0.0717)	(0.0514)	(0.114)
Pct young siblings in HCB	0.0936	-0.0827	-0.00440	0.162*	0.0197	0.143**	-0.0506	0.0106
	(0.180)	(0.137)	(0.0418)	(0.0873)	(0.0649)	(0.0623)	(0.0600)	(0.132)
	IV				*IV*			
Any young sibling in HCB	0.0470	1.356	0.481	-0.550	-0.149	0.00116	-0.293	0.344
	(1.036)	(1.667)	(0.355)	(0.472)	(0.333)	(0.422)	(0.298)	(0.953)
Pct young siblings in HCB	0.0519	1.499	0.532	-0.608	-0.173	0.00135	-0.340	0.400
	(1.147)	(1.864)	(0.390)	(0.521)	(0.387)	(0.490)	(0.354)	(1.119)
Nobs	2,612	2,612	2,612	2,612	2,163	2,164	2,164	2,164

Notes: Standard errors in parentheses are clustered at the municipality level. The units for time to HCB are minutes divided by 100. The regressions include child and household characteristics such as child's age and gender, the mother's and the head of the household's age and gender, and indicators for the composition of the household (number of persons 0-5 years old, 6-10,11-15, 16-17, 18-39, 40-59 and 60 years old or more). A household wealth index is also included, constructed based on the household's assets and appliances information. Municipality characteristics include having a hospital in town, percentage of households with pipe water and percentage with sewage connection. I also control for household location variables, such as living in the center of the town and time to the health center, the school and the town hall. Other variables include department (state) fixed effects and indicators for the geographic region in Colombia (four in total)..

Table 29 Effect of HCB on older sisters' education and time use

Panel A. Effect on education outcomes

	Enrolled (this year)	Enrolled (last year)	Passed (last year)	Failed (last year)	Drop out (last year)	Absent 20-40 days (last year)	Absent 40+ days (last year)
				OLS			
Any young sibling in HCB	0.0123	0.0613**	-0.00979	0.0628	0.0148	0.0225	0.0127
	(0.0272)	(0.0292)	(0.0671)	(0.0433)	(0.0149)	(0.0186)	(0.0142)
Pct young siblings in HCB	0.00384	0.0516	-0.000523	0.0392	0.0194	0.0287	0.0177
	(0.0293)	(0.0341)	(0.0788)	(0.0444)	(0.0194)	(0.0224)	(0.0183)
				IV			
Any young sibling in HCB	0.106	-0.0540	0.00661	-0.0126	-0.0329	0.0538	0.0192
	(0.205)	(0.263)	(0.216)	(0.141)	(0.0615)	(0.0598)	(0.0290)
Pct young siblings in HCB	0.117	-0.0599	0.00733	-0.0139	-0.0364	0.0596	0.0213
	(0.229)	(0.290)	(0.239)	(0.156)	(0.0673)	(0.0671)	(0.0324)
Nobs	1,207	1,207	1,207	1,207	1,207	1,207	1,207

Table 29 Effect of HCB on older sisters' education and time use

Panel B. Effect on Time Use

	Day before				Extra-school hours			
	N. hours work	N. hours school	N. hours homework	N. hours housework	N. hours work	N. hours housework	N. hours homework	N. hours rest
	OLS				*OLS*			
Any young sibling in HCB	-0.0106	-0.0345	-0.00588	0.197*	0.0508	0.222**	-0.126*	0.185
	(0.0907)	(0.213)	(0.0709)	(0.102)	(0.0615)	(0.111)	(0.0727)	(0.162)
Pct young siblings in HCB	-0.0141	-0.0653	-0.00873	0.249*	0.0496	0.243**	-0.151*	0.173
	(0.0917)	(0.258)	(0.0767)	(0.128)	(0.0716)	(0.110)	(0.0805)	(0.190)
	IV				*IV*			
Any young sibling in HCB	-0.307	2.566	0.543	-1.195	-0.0238	1.075***	-0.137	0.562
	(0.781)	(1.788)	(0.357)	(1.279)	(0.421)	(0.384)	(0.525)	(0.926)
Pct young siblings in HCB	-0.340	2.842	0.601	-1.323	-0.0277	1.252***	-0.160	0.655
	(0.870)	(2.033)	(0.391)	(1.445)	(0.490)	(0.464)	(0.610)	(1.077)
Nobs	1,207	1,207	1,207	1,207	1,040	1,041	1,041	1,041

Notes: Standard errors in parentheses are clustered at the municipality level. The units for time to HCB are minutes divided by 100. The regressions include child and household characteristics such as child's age and gender, the mother's and the head of the household's age and gender, and indicators for the composition of the household (number of persons 0-5 years old, 6-10,11-15, 16-17, 18-39, 40-59 and 60 years old or more). A household wealth index is also included, constructed based on the household's assets and appliances information. Municipality characteristics include having a hospital in town, percentage of households with pipe water and percentage with sewage connection. I also control for household location variables, such as living in the center of the town and time to the health center, the school and the town hall. Other variables include department (state) fixed effects and indicators for the geographic region in Colombia (four in total).

Table 30 Effect of HCB on older brothers' education and time use

Panel A. Effect on education outcomes

	Enrolled (this year)	Enrolled (last year)	Passed (last year)	Failed (last year)	Drop out (last year)	Absent 20-40 days (last year)	Absent 40+ days (last year)
				OLS			
Any young sibling in HCB	0.0182	0.0122	0.0392	-0.0459	0.0190	0.0272	0.0143
	(0.0237)	(0.0475)	(0.0426)	(0.0329)	(0.0138)	(0.0203)	(0.00959)
Pct young siblings in HCB	0.00518	0.00591	0.0380	-0.0568	0.0251	0.0282	0.0183
	(0.0293)	(0.0582)	(0.0493)	(0.0397)	(0.0175)	(0.0247)	(0.0119)
				IV			
Any young sibling in HCB	-0.172	-0.263	-0.131	-0.270	0.0849	-0.151	0.00965
	(0.242)	(0.211)	(0.266)	(0.288)	(0.117)	(0.225)	(0.117)
Pct young siblings in HCB	-0.190	-0.290	-0.144	-0.298	0.0937	-0.166	0.0107
	(0.263)	(0.233)	(0.290)	(0.325)	(0.134)	(0.241)	(0.130)
Nobs	1,405	1,405	1,405	1,405	1,405	1,405	1,405

Table 30 Effect of HCB on older brothers' education and time use

Panel B. Effect on Time Use

	Day before				Extra-school hours			
	N. hours work	N. hours school	N. hours homework	N. hours housework	N. hours work	N. hours housework	N. hours homework	N. hours rest
		OLS				*OLS*		
Any young sibling in HCB	0.216	-0.0462	0.0100	0.00821	0.0236	0.0251	0.0319	-0.167
	(0.230)	(0.144)	(0.0386)	(0.0584)	(0.0627)	(0.106)	(0.0563)	(0.120)
Pct young siblings in HCB	0.254	-0.131	0.00623	0.0263	0.00808	0.0194	0.0452	-0.160
	(0.292)	(0.159)	(0.0350)	(0.0690)	(0.0757)	(0.102)	(0.0692)	(0.142)
		IV				*IV*		
Any young sibling in HCB	0.220	0.178	0.492	-0.375	-0.110	-0.946	-0.225	0.211
	(1.373)	(1.716)	(0.472)	(0.593)	(0.413)	(0.743)	(0.382)	(1.149)
Pct young siblings in HCB	0.242	0.196	0.543	-0.414	-0.128	-1.099	-0.262	0.245
	(1.522)	(1.897)	(0.515)	(0.675)	(0.486)	(0.873)	(0.453)	(1.346)
Nobs	1,405	1,405	1,405	1,405	1,123	1,123	1,123	1,123

Notes: Standard errors in parentheses are clustered at the municipality level. The units for time to HCB are minutes divided by 100. The regressions include child and household characteristics such as child's age and gender, the mother's and the head of the household's age and gender, and indicators for the composition of the household (number of persons 0-5 years old, 6-10,11-15, 16-17, 18-39, 40-59 and 60 years old or more). A household wealth index is also included, constructed based on the household's assets and appliances information. Municipality characteristics include having a hospital in town, percentage of households with pipe water and percentage with sewage connection. I also control for household location variables, such as living in the center of the town and time to the health center, the school and the town hall. Other variables include department (state) fixed effects and indicators for the geographic region in Colombia (four in total).

Table 31 Effect of HCB on female labor supply

	Currently working	N. hours work per week	Nmonths work last 12 months	N. hours work per week	Nmonths work last 12 months
		(Unconditional on working)		(Conditional on working)	
			OLS		
Any young child attending HCB	0.266***	8.933***	2.765***	-2.015	0.0664
	(0.0609)	(2.849)	(0.799)	(2.501)	(0.694)
Percentage young children HCB	0.286***	10.88***	2.930***	0.993	0.0614
	(0.0702)	(3.474)	(0.945)	(2.676)	(0.815)
			IV		
Any young child attending HCB	0.851**	41.16**	9.313**	29.59*	2.461
	(0.382)	(16.76)	(4.441)	(16.64)	(2.890)
Percentage young children HCB	0.976**	47.21**	10.69**	32.04*	2.659
	(0.448)	(19.34)	(5.180)	(17.20)	(3.119)
Nobs	2,047	2,027	2,043	734	748

	Any young child in HCB	Percentage young children in HCB
First stage-Fstat	6.37	6.12

Notes: Standard errors in parentheses are clustered at the municipality level. The units for time to HCB are minutes divided by 100. The regressions include child and household characteristics such as child's age and gender, the mother's and the head of the household's age and gender, and indicators for the composition of the household (number of persons 0-5 years old, 6-10,11-15, 16-17, 18-39, 40-59 and 60 years old or more). A household wealth index is also included, constructed based on the household's assets and appliances information. Municipality characteristics include having a hospital in town, percentage of households with pipe water and percentage with sewage connection. I also control for household location variables, such as living in the center of the town and time to the health center, the school and the town hall. Other variables include department (state) fixed effects and indicators for the geographic region in Colombia (four in total).

Figure 14 Age distribution of the sample

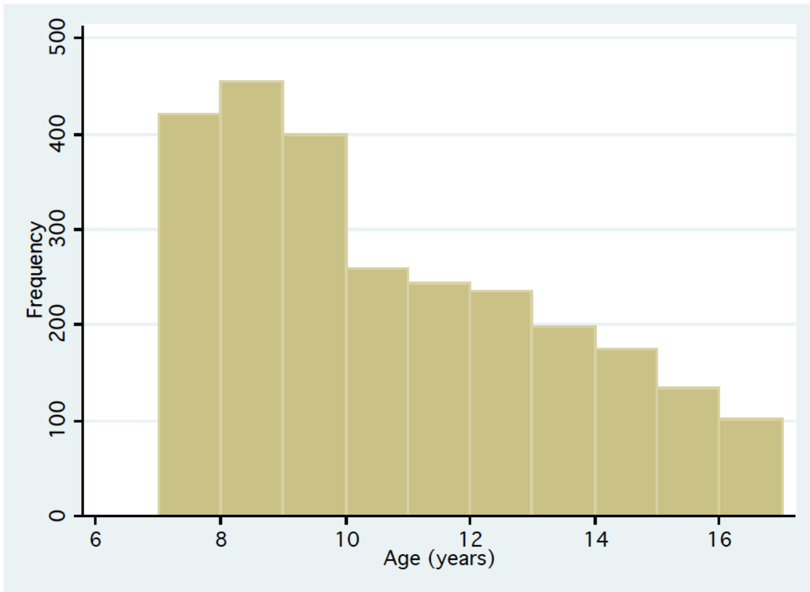


Figure 15 Participation in HCB, by older siblings' age and gender

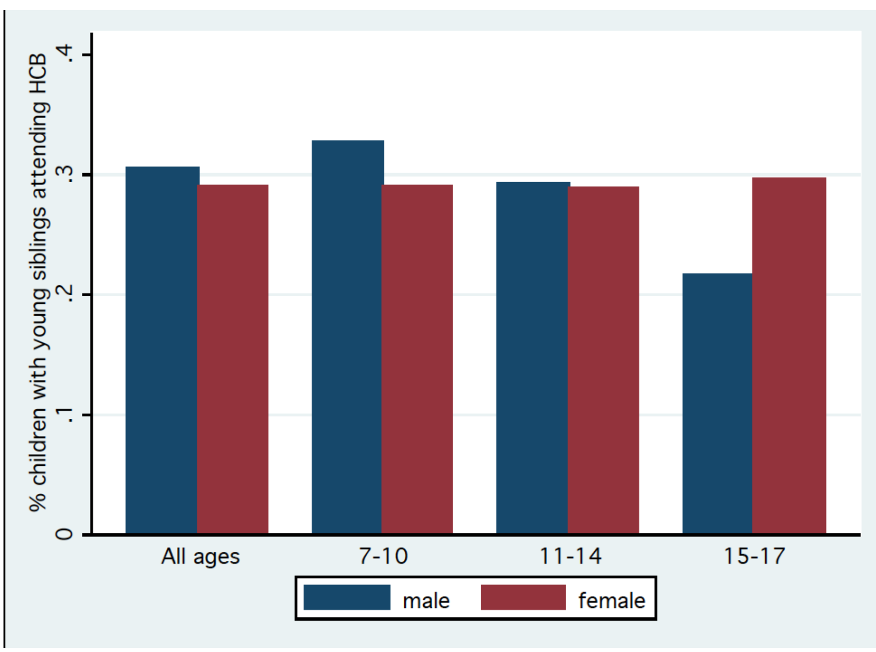


Figure 16 Time to HCB, by participation and gender

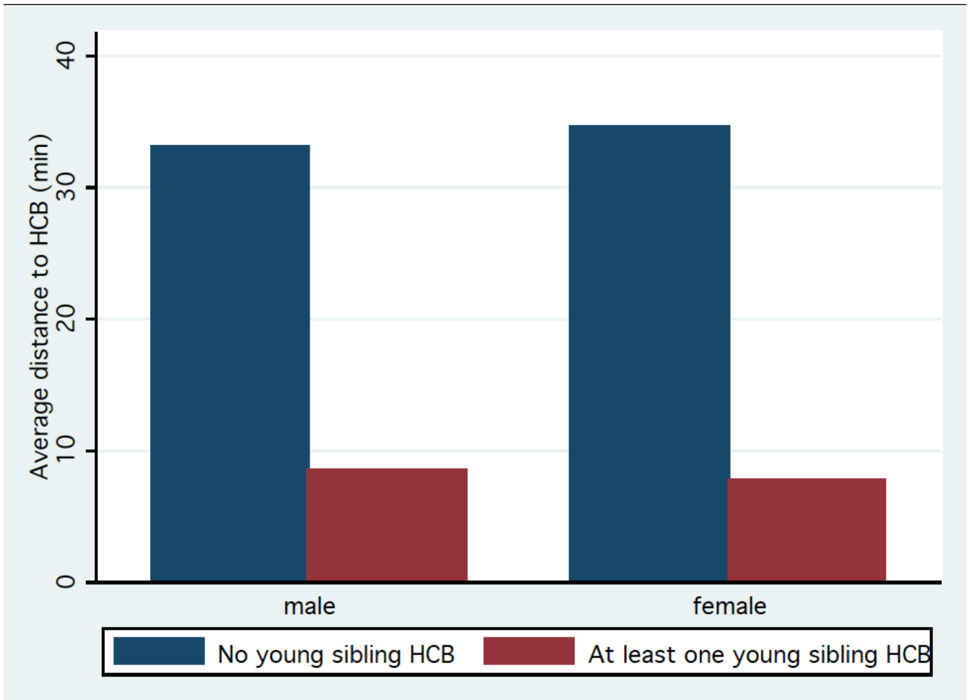


Figure 17 Participation in HCB, by income quintile

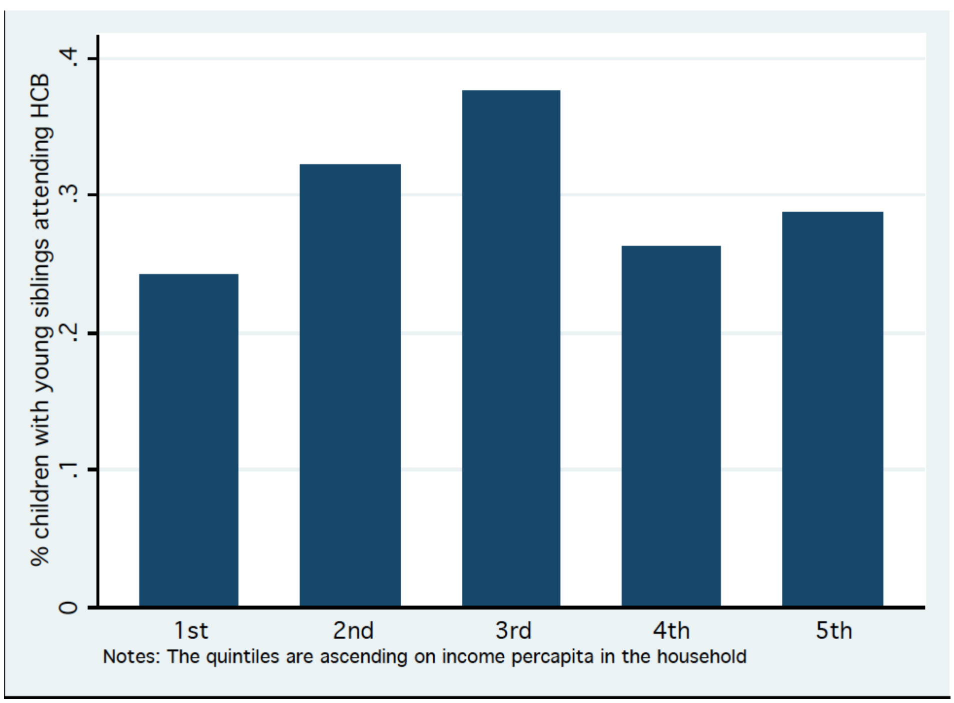
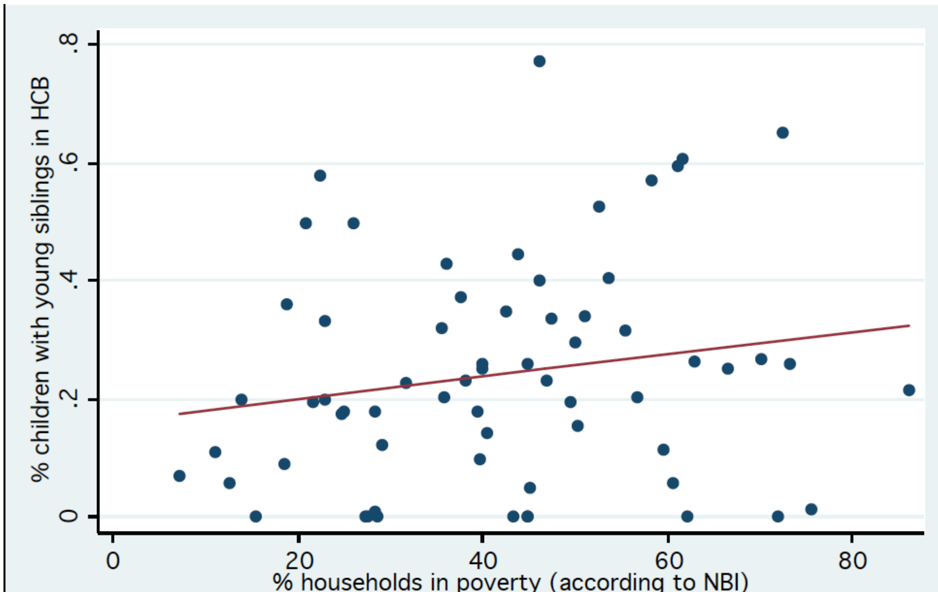


Figure 18 Participation in HCB vs. municipality poverty



Notes: The NBI (Basic needs index) counts the percentage of households with at least one of the following: inadequate housing or sanitary services, overcrowded homes, high economic dependence to the head or with children not attending to school